AUSTRALASIAN JOURNAL OF PHILOSOPHY

Edited by A. K. STOUT

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BRIAN ELLIS: Some Fundamental Problems of Indirect Measurement.

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REVIEWS, BOOKS RECEIVED, NOTES AND NEWS.

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PRAGMATICS AND INTENSION

By W. MAYS

I

Carnap, in an endeavour to meet Quine's objections to his definition of the concepts of 'synonymy' and 'analyticity', has tried to set up a method of testing intensions pragmatically.[1] For Carnap, not only can we ascertain empirically the extension of a predicate, *i.e.* the objects it denotes, but we can also by empirical methods determine its intension. He maintains that we can state a behaviouristic procedure for the determination of conceptual meaning. As he puts it, "the assignment of an intension is an empirical hypothesis which, like any other hypothesis in linguistics, can be tested by observations of language behaviour".[2] We can, for example, enumerate the possible meanings of a word (in the form of descriptions), asking the individual to select the one he thinks to be most appropriate. In this way Carnap believes we can in some measure determine the precise meanings given by individuals to the words they use.

Carnap, however, still perpetuates the sharp distinction between analytic and synthetic, overlooking thereby that this is just the point under discussion. Quine's criticisms do not for him in any way concern the formal correctness of pure semantics, as Carnap regards semantic concepts as having some sort of independence and as being fruitful on their own account. Quine, he assumes, is rather doubting whether there are any clear and fruitful corresponding pragmatic concepts which could serve as explicanda for the semantic concepts, since "without this pragmatical substructure, the semantical intension concepts, even if formally correct, are arbitrary and without purpose".[3] Quine's position, however, would seem to be that, whatever significance semantic concepts have, they can (or ought) always be formulated

[1] R. Carnap, *Meaning and Synonymy in Natural Languages*, Philosophical Studies, April, 1955
[2] *Ibid*, p. 37
[3] *Ibid.*, p. 35.

behaviourally. On such a view, pure semantics cannot be sharply differentiated from pragmatics. He is thus not simply asking, as Carnap implies, for the production of the corresponding pragmatic concepts.

Carnap's contention that there is an empirical method for testing intensions no doubt stems from his view that a concept (or an intension) is something objective.[4] He tells us, for example, that the intension of the word *blau* in German is the property of being blue. Carnap seems to assume that over and above the objects in the real world there is a system of such possible properties which gives meaning to our language. He tells us that, unlike the case of the extension of a predicate, there are more than one and possibly infinitely many properties which the same predicate can express. To illustrate this he postulates two linguists unfamiliar with German who are studying the language habits of a German-speaking person. Carnap argues that though they may come to agree that a certain predicate denotes a particular object for that person, whom we may call Karl, they may still disagree as to the precise meaning it has for him. Thus the assignment of an intension to a predicate becomes for the two linguists an empirical hypothesis. It has this character since (a) they cannot know precisely what intension Karl has in mind when he applies this predicate, and (b) the intensions postulated are regarded as selections from such a system of possibilities.

Carnap, who regards language on a pragmatic level as a system of dispositions for the use of expressions, defines the ability of an individual to apply the right predicate to the right property (*i.e.* what it means for a predicate to have an intension) as a "disposition of ascribing the predicate 'Q' to any object y if and only if y has the property F"[5] (where F is an observable property). However, a strict univocal relationship of this sort is found only in logic and not in our everyday language. The similarity of the phrase 'if and only if' as it occurs in this type of statement, which implicitly refers to an empirical subject-matter, to its use in such a formula as '$p \equiv q$' is deceptive. By its use Carnap seems to want to transform an empirical relationship which only holds with a degree of probability into a formal logical one.

Further, this type of definition assumes, for example, that, if a blue object is presented to a subject (whose current language is English), he will tend to respond with the word 'blue'. We seem

[4] The word 'concept' is used by Carnap as a convenient common designation for properties, relations and functions. He emphasises that it does not refer to terms, *i.e.* words or phrases, but to their meanings, and it does not refer to the actual mental characteristics of conceiving but to something objective.

[5] *Ibid.*, p. 42.

to deal with a conditioned reflex theory of how predicates become attached to properties. There is, however, no such necessary restriction on the kind of object which can cause a particular verbal response to occur. It will tend to vary with the experience of each individual, which is usually radically different from the standardised environment of the laboratory animal. Further, the responses of a poet or a lunatic may show still more striking divergencies. This becomes much more evident when we deal with words referring to complex states of affairs, rather than simple colours. As Carnap seems to recognise himself, when he talks about "intensional vagueness", an expression is normally used not in a sharp and precise manner, but over an established range of situations. Nevertheless, he does seem to assume that we have clear cases of objects having properties, and that "intensional vagueness" arises from an inadequate understanding of them on our part.

When, on the other hand, Carnap speaks of the two linguists in their study of Karl's linguistic behaviour reaching complete agreement as to the extension of a given predicate, say *blau*, he seems to slur over the complexity of this process of reaching agreement. Both the German and English languages have a reference to a similar type of Western culture which acts as a common ground between them. As Quine points out, the first moves in understanding a strange language are at bottom a matter of exploiting the overlap of our cultures. If we were dealing with the language of some primitive tribe with bizarre customs, or with beings from outer space equipped with different types of sense-organs, agreement might not be so easy. But in all cases agreement would be reached on the assumption (except perhaps in the case of space-beings) that we were both referring to a common world of objects. It is this among other things which helps us to set up a correspondence between *blau* and 'blue'. The class of objects to which Karl applies *blau* tends to coincide with the class of objects to which we apply 'blue'.

Further, the linguist's interpretation of Karl's language behaviour is dependent upon the assumption that he is not dealing with isolated predicates, but with predicates which are systematically related together. Without some acquaintance with other predicates of the language being studied, the linguist would be unable to determine the precise extension of a single predicate. Indeed, to do this he would have to be acquainted with a substantial part of the language under interpretation. Something similar occurs in the genetic acquisition of a language. Children when they learn a language do so largely through imitating the complex conversation of adults, and not the simplified predicate

form exhibited in the Carnapian sentence. Further, this emphasis on the predicate as one of the key notions of language seems to be more a peculiarity of some Indo-European languages than of language in general. Hence, even before the precise nature of the unknown language has been determined, Carnap assumes that it will have a fairly close resemblance to the syntactical structure of the linguists' own language, presumably English, an assumption which no self-respecting philologist could afford to make. This point is overlooked by Carnap since the unknown language is in this case German, which is in fact Indo-European. But he is, however, not justified in arguing from a special case to language in general. Carnap's whole approach to this question assumes something like a general universal language of predicates, of which the particular natural languages are to be regarded as instances, or, putting it in its traditional form, he assumes a common universe of thought. Carnap would certainly find himself in difficulties if he were investigating a language in which the meaning of the expressions changed from context to context. This, we are told, is the case with some of the more exotic languages spoken, e.g., in the Pacific islands. Malinowski, who studied these languages, was therefore led to stress the instrumental character of a language in contrast to its designative function.

II

For Carnap, then, the intension of a predicate will include not only actual but also all possible cases. Thus he says, "The intension of a predicate may be defined as its range, which comprehends those possible kinds of objects for which the predicate holds".[6] We may apparently determine this range by starting extensionally from some given specimen denoted by the predicate, and by suitable questioning of the subject find out what variations in shape, size and colour he will admit and still apply the same predicate. Having in this way determined the intension of a predicate, we can proceed to define the notion of 'analyticity' in terms of it (intension seems here to be defined in terms of the logically possible) as follows: "A sentence is *analytic* in L for X at t if its intension (or range or truth-condition) in L for X at t comprehends all possible cases".[7] In other words, a sentence is analytic in a language L at a particular time t for a subject X if it is true solely in virtue of its cognitive meaning, which seems to be taken as self-evident. Synonymy is defined on similar lines: "Two expressions are *synonymous* in the language L for X at

[6] *Ibid.*, p. 39
[7] *Ibid.*, p. 42.

time t if they have the same intension in L for X at t".[7] In other words, they are synonymous if they have the same cognitive meaning.

Carnap then believes that 'analyticity' and 'synonymy' as pragmatic concepts, defined in terms of linguistic behaviour, provide a practical justification for the corresponding concepts of pure semantics. However, Carnap's account would seem to be based rather on abstract logical models than on first-hand empirical linguistic investigations. Carnap presents us with a contemplative picture of knowledge in which the 'subject' is in some way assumed to be directly aware of intensions, conceived of as ranges of actual and possible properties, which he can report on when suitably interrogated. Further, the attempt to justify 'analyticity' and 'synonymy' on the pragmatic level by an appeal to possibility does not help us. Carnap has been seeking for a justification of the concepts upon which our logic is based, namely 'synonymy' and 'analyticity'. This he does by defining them in terms of systems of possible objects. Such systems, however, presuppose the very concepts he is trying to explain, since they are certainly "grounded on meanings independent of fact", which is what we mean by analyticity.

Carnap endeavours to clarify the difference between the extensional and intensional aspects of a word by comparing the entry (a) *Pferd,* horse, in the notebook of one of our hypothetical linguists studying the German language with the entry (b) *Pferd,* horse or unicorn, in the notebook of the other linguist. Carnap wishes to discover whether we have an empirical method for confirming the truth of either (a) or (b). He points out that as unicorns do not exist the two intensions ascribed to the word *Pferd,* though different, have the same extension—they refer to the same object. Hence, he argues, if the extensionalist theory were right there would be no way of empirically deciding between them.

To this we might reply that the above criticism only holds if the extensionalist's universe of discourse is restricted in the way Carnap restricts it, so as to include only spatio-temporal objects. It certainly would not apply if we took into account the world of our imagination. But even on a purist extensionalist approach one could always produce a picture of a unicorn as it appears, for example, in the British royal coat of arms. As a consequence of his circumscribed view of extension Carnap would have to regard even the picture of this mythical animal as being intensional in character. Nevertheless, the average citizen of the British Isles is usually more familiar with unicorns, as inhabiting

the world of mythology, than he is with some existing zoological specimens.

Carnap now outlines a method for deciding the truth of entries (a) and (b) in the linguists' notebooks. The linguist, in studying the German language (by observing the language behaviour of Karl), can begin his studies by making out the extensions of such predicates as *blau* and *hund*. With this simple vocabulary (acquired perhaps by pointing to the object, seeing whether Karl nods his head, etc.) he can go on to determine intensions by describing possible cases. He may, to quote Carnap, describe a unicorn (in German) as "a thing similar to a horse, but having one horn in the middle of the forehead". The linguist can then ask Karl whether he is willing to apply the word *Pferd* to it. A reply in the affirmative or negative will then confirm or disconfirm either (a) or (b).

Carnap believes that the extensionalist might perhaps object that the man-in-the-street is usually unwilling to say anything about non-existent objects. But as he himself points out (without drawing the obvious moral) the man-in-the-street is well able to answer questions about assumed or even non-existent situations. This is shown in ordinary conversations about alternative plans of action, about dreams, legends and fairy tales. However, the ability of the ordinary man to cope with alternatives largely arises from his acceptance of the world of imagination as an important part of his experience. The elements of the latter are for him not always sharply separated from those occurring in the external world. As we know only too well, myths play an important part in our culture.

Carnap's views on the nature of 'extension' bear some resemblance to those of Russell, when the latter argues that since one cannot name a person who is not there, the only way of dealing with mythological persons such as 'the present King of France' and *Romulus* is by the method of descriptions. Of this Gardiner has said, "To refute Russell's view that you cannot name a person who is not there, it is necessary only to quote *Romulus* as evidence that you can. And the thousands of fictional and mythological characters which we may remember will not improve upon that answer".[8]

---

[8] Sir Allan H. Gardiner, *The Theory of Proper Names* (Second Edition), Oxford, 1954, p. 66. The feature of descriptions which endear them so much to Russell is that they enable us to deal with the names of non-existing personages. Such names do not according to him refer to spatio-temporal objects, but simply imply that one and only one thing satisfies a certain condition.

III

Carnap tells us that we can discover whether a given person has a particular linguistic disposition for the use of a specific predicate in two ways: (a) by producing the condition and seeing whether the response occurs (behavioural or operational analysis); (b) by describing the person's physiological state and with the aid of the appropriate laws calculating the linguistic responses he would make to any specified changes in his environment (structural analysis). But Carnap wisely admits that in the present condition of physiological knowledge (b) is not a very practicable method. He compares such a linguistic disposition with the ability of an automobile to accelerate on a horizontal road at a specific speed (*e.g.* ten miles per hour). The hypothesis that the automobile has this ability may, he argues, be tested either by driving the car and observing its performance, or by studying its internal physical structure and calculating the acceleration which would result under the specified conditions.

However, in the case of the automobile one knows exactly what an acceleration is. We know something about the internal structure of the motor and the mechanical laws it follows. Further, our calculations can be verified within a small degree of probable error. But a linguistic disposition, for example, to say 'cow' when one is presented, seems to be an entirely different matter. We have no guarantee that the response will occur when the stimulus is given. We deal with a complex physiological system of which we do not know whether a structural analysis (in terms of micro-physical laws) is even theoretically possible. Carnap's view implies something like a preconceived conception of the nervous system as a digital (or analogue) computer and would appear to be another version of the physicalism which one thought he had abandoned—namely that psychological statements could be translated into physical ones.

Carnap now comes to his *pièce de résistance* and proceeds to consider how we could investigate pragmatically the intensions assigned to its language by a robot. Unlike the case of a human being, it is assumed that we have a sufficiently detailed blue-print according to which the robot is constructed. Carnap endows this robot (which seems to be a 'knowledge by acquaintance and description' machine) with the abilities of organisation and the use of language, and gives it three input organs *A.B.C.* and an output organ. The function of *A* is to observe visually presented objects, *i.e.* recognise objects, that of *B* to receive a general description, *i.e.* a linguistic description of an object, and that of *C* to receive a predicate. The output organ can either affirm, deny

or abstain from responding, in accordance with whether or not the predicate applies to the presented object or description. The machine abstains from answering when it is, as it were, unable to 'make up its mind' whether or not the predicate applies, usually when the presented information is inadequate. Carnap does not tell us very much about the internal structure of the robot or what he means here by 'abilities of organisation and the use of language'. We can only surmise that it would cover at least learning and the ability to symbolise visual patterns by linguistic ones. He thus conjures up a robot of a rather advanced type, of which no models as yet exist. Without some plan of its underlying principles, we remain very much in the dark as far as its method of functioning is concerned.

Carnap now applies to the robot the method of deriving intensions by behavioural analysis. He assumes that we can begin to investigate its language extensionally by presenting objects at *A* and recording its responses. On the basis of such a preliminary survey we then present predicate expressions (descriptions) at *B*, using only these interpreted signs. In the light of the given responses we may determine what intensions the robot assigns to the words of its language. Or to put it differently, as when previously investigating Karl's linguistic behaviour, we begin by first pointing to objects, and after having arrived at a minimum vocabulary proceed to ask further questions in terms of descriptions formed from it. We test here, as it were, the machine's cognitive rather than perceptive behaviour, and in this way discover what meaning the sentences of its language have for it.

If on the other hand we want to use the method of 'structural analysis' to determine the 'intensions' of the robot, we need first, according to Carnap, to equip ourselves with a blue-print (describing the material state of the robot at time *t*). We can then, using the appropriate physical laws determining the robot's functioning, calculate the response it would make (Yes, No, ?) to various possible inputs in the form of visual objects, which could be given *via A*. If we begin with, say, the predicate *Q* (presented at *C*) and the observable properties F_1 and F_2, we may find by calculation (much as we can calculate the acceleration of the automobile) that we will get "an affirmative response if and only if an object having the property F_1 is presented at A, and a negative response if and only if an object with F_2 is presented at *A*".[9] For Carnap this result indicates that the boundary of *Q*'s intension lies somewhere between the boundary

[9] *Ibid.*, p. 44.

of F_1 and F_2, which are presumably taken as incompatible in character. This seems to be a roundabout way of saying that the predicate Q refers to F_1, which determines its intension.

In this preliminary determination of intensions, the intensional boundaries between predicates may not always, we are informed, be precisely specified. There may be a zone of indeterminateness between them, *i.e.* a certain amount of vagueness and indetermination in their application to specific properties. Carnap recognises that for some predicates, for example those representing colours, the zone of indeterminateness may be quite small, *i.e.* the number of '?' responses will be low. In this case our preliminary determination of the intensions of the robot's language will be fairly precise.

Carnap gives one the impression here that such 'intensional vagueness' is somehow due to our inability to grasp the precise significance of the predicates obtained in this sample of the robot's language. However, this would only hold in the case where the robot's language was a completely strange one to us. But it would not apply if the language was one we already understood, *e.g.* English. In this case, vagueness in the machine's application of predicates to specific properties would have to be traced to a deficiency in its own powers of visual analysis[10] (which in a human being we might term inadequate concept formation). For Carnap the boundaries between properties would appear to be clear-cut, and the properties themselves discrete and objective. If the machine was fitted with a more adequate visual analyser (or perhaps learning organ) it would manifest a greater *finesse* in its application of predicates to properties.

Having obtained a sample of the robot's vocabulary (by calculations concerning input A), the investigator can, according to Carnap, proceed and make calculations concerning descriptions (constructed out of this vocabulary) which could be presented at B. The following result, we are told, may be obtained for a predicate P and properties G_1 and G_2. If we present the predicate P at C and any description D constructed from this sample vocabulary at B, the robot will give "an affirmative response if and only if D (as interpreted by the preliminary results) logically implies G_1, and a negative response if and only if D logically implies G_2".[11] This apparently means that the intension of P is determined by the description D which is, as it were, an inflated description of G_1 which it may be taken as symbolising. In this way, Carnap argues, we can calculate the robot's responses

[10] And this would certainly have to show itself in the form of a certain imprecision in the calculations we make.

[11] *Ibid.*, p 44

to specific descriptions, and thereby obtain a more comprehensive account of its language and the meaning assigned to it.

Since the machine, like the human being, manifests 'intensional vagueness'—a certain imprecision in the drawing of boundaries between the intensions of predicates—this would show itself in a corresponding imprecision in a certain sub-class of our calculations. And to this sub-class we would have to assign the value '?'.

Returning to the actual functioning of the machine we then see that it employs a rigid trichotomy in its responses (Yes, No, ?) rather like that of a three-valued logical system. It replies 'Yes' when it can distinguish a property corresponding to the applied predicate (or description), 'No' when the predicate is incompatible with the distinguished property, and '?' when it is unable to decide where to draw the boundary of the predicate's application. But what clearly emerges is that in all these cases both predicate and descriptions refer to some sort of entity or property which gives them their intension.

It should be pointed out, however, that in the case of human beings, instead of this trichotomy we would get something more like a continuum of responses, as is the case, for example, when we grade or rank extensive magnitudes. Further, there will be a certain amount of arbitrariness as to where we draw the line between properties, as they usually blur into each other. Our boundary decisions will also tend to vary in accordance with the contextual situation in which we find ourselves. As Strawson puts it when speaking of predicates, "The boundaries are more like areas of indeterminateness than frontier-lines".[12] Carnap, on the other hand, conceives properties as being independent and discrete. He therefore resolves 'intensional vagueness' into something like 'subjective error', which it is assumed prevents us as well as the robot from grasping the precise boundaries of properties as they exist in nature (or as possibilities).

According to Carnap, we can also discover what sort of reply the machine would give to descriptions referring to non-existent objects and causally impossible ones. The robot could be presented with descriptions of unicorns, minotaurs and other mythical beasts. We might, for example, ask whether we can apply the predicate 'man' to a minotaur and obtain a '?' response, since an appropriate decision procedure for resolving this classificatory problem may not have been built in. It might even be confronted with a blueprint of itself, and asked to decide whether it is a 'thinking being'. By giving it a large number of such border-line concepts, we might involve the machine in grave difficulties.

[12] P. F. Strawson, *Introduction to Logical Theory* (1952), p. 5.

IV

In some ways this mechanical approach seems to be a reversion to Carnap's earlier position,[13] when he asserted that the meaning of a word can be specified by means of formal rules, either by (a) translation, or (b) definition. An example of (a) would be "cheval = horse", and of (b) "elephant = animal possessing such and such pachydermous characteristics". However, in both cases we fall back on synonymy, which is certainly not a formal rule (as far as ordinary language is concerned). This can only be the case in an artificial semantic system in which it is arbitrarily defined as one. The 'logical implication of a property by a description', as seen *e.g.* in Carnap's account of his method for computing the intensions of a robot, is simply a formal translation of this kind. The difference seems to be that here the synonymy relation is built into the machine (whose operations can be determined by calculation) instead of occurring as a feature of a linguistic statement. There is thus no radical difference from his earlier position that the meaning of a word can be specified by formal rules. The blue-print of the robot does not then seem to differ essentially from that of an abstract semantic system. The only difference is that the postulates and definitions are now built into the machine.

Carnap's belief, that the concept of intension can be applied even to a robot, does imply that the understanding of the sense of a word is not an essential element in its meaning for him. This no doubt is due to Carnap's assumption that the relation between a predicate and a property (which determines the predicate's intension) is an objective one. But even if we accept the view that concepts have an objective character, can we talk of a statement as having an "intension", unless it is used by someone who understands its significance? We are not merely concerned with the correct application of a predicate to a specific property (which could occur by chance), but also with our understanding of the significance of the predicate. This is one of the reason's why some philosophers, such as G. E. Moore, have insisted on the difference between "uttering" and "asserting" a statement, where "asserting" implies understanding. Though a robot might utter the statement "London is large", it would not be asserting it in this sense. However well-organised its vocabulary might be, we should not usually say that it understood the significance of the words it was uttering.

Further, in recent years the sharp distinction between names and descriptions (the extension-intension dogma), on which

[13] *Cf* R. Carnap, *Unity of Science*

Carnap's hypothetical robot is based, has fallen into some disrepute. This distinction, it is pointed out, arises from the assumption that the meaning of an expression is identical with the object to which it refers or applies. As descriptions of mythical objects such as Pegasus, which do not refer to existing objects, are nevertheless meaningful, it is assumed that they must be about something. But, since it is not wished to make their meaning depend on anything psychological, they are taken as predicates referring to logical entities, which are symbolised by existential quantifiers. Nevertheless, as Strawson, for example, points out, this requirement, that for a predicate to have a meaning it must refer to a specific individual, is not fulfilled even with proper names. The same name can be applied to many different persons and things. Further, for a name or a description to be significant, it is not necessary that the object named or described should exist. It is sufficient if one can describe or imagine circumstances in which its use would make it true or false.[14]

As Gardiner puts it, "for Language it is a matter of complete indifference whether the thing named or described has or once had an external existence, or rather . . . whether or not it has ever come before its namer or describer".[15] Gardiner is of the opinion that "the function of Language is purely instrumental, and that, provided words can be found to make a listener think of something to which the speaker wishes to make reference, it matters not whether a proper name is used or a description comprising several words. The words are mere scaffolding to be removed when its purpose is fulfilled".[16] His point then is that in communication we are not primarily concerned with the logical structure and the symbolism of the language we use, but with its psychological effectiveness in bringing about a certain result.

Once again Carnap seems to base his position on the view that logical and mathematical systems are good models for ordinary language. He seems to be arguing by analogy that just as mathematical symbols are taken as standing for precise concepts, so the words of a language (categorematic and syncategorematic) have a similar referential character, though they refer not merely to concepts but also to things. Carnap thereby overlooks the difference between a language in use, which may not have a precise referential function, and a mathematical system, which on the logistic view usually has.

Manchester University.

[14] *Cf.* Strawson, *ibid.*, p. 185.
[15] *Ibid*, p. 66.
[16] *Ibid*, p 61.

SOME FUNDAMENTAL PROBLEMS OF INDIRECT MEASUREMENT

By BRIAN ELLIS

In a paper published recently in this journal,[1] I made a distinction between direct and indirect measurement, and discussed certain problems of direct measurement. It was argued that most, if not all, of the important directly measurable quantities are measured by logically similar procedures. Such quantities were described as fundamentally measurable quantities, and the procedures as fundamental measuring procedures. But since few quantities are measurable by the same procedures over the whole of their range, this terminology was perhaps somewhat misleading. Ordinary distances may be fundamentally measurable, but subatomic and interstellar distances are not. The distinction I wished to make was not so much a distinction between different sorts of quantities, as a distinction between different forms of measurement —direct and indirect measurement.

I now wish to discuss certain problems which arise in connection with indirect measurement, i.e. measurement which depends upon prior measurement. But first it should be noted that measurement may depend upon prior measurement in different ways. One way in which it may do so is illustrated by temperature measurement. The measurement of this quantity always depends upon the measurement of some other quantity which in special circumstances is taken as a quantitative criterion for temperature. Likewise, the measurement of pressure always depends upon the measurement of something else. Where such is the case, where the measurement of a quantity p always depends upon the measurement of some other quantity or quantities, p will be described as a "derived magnitude" and the measurement of p will be said to be a form of "derived measurement".

A second way in which measurement may depend upon prior measurement is illustrated by the measurement of very large distances, masses and time intervals. Such measurement, like derived measurement, depends upon the adoption of a quantitative criterion for the quantity p to be measured, but it also depends, to some extent, upon the direct and fundamental measurement of p in a more restricted range. Measurement of this kind, which seems to straddle fundamental and derived measurement, will here be described as "mixed measurement".

I do not know in what other ways measurement may depend

[1] May, 1960.

upon prior measurement. But in this paper I propose to discuss some problems which arise in connection with these two forms of indirect measurement.

I. *Derived Measurement*

I will limit my discussion of derived measurement to temperature measurement, although I think that most of what I have to say about temperature measurement is applicable to other forms of derived measurement.

First, it should be noted that the problems involved in setting up a scale of temperature are somewhat different from those involved in setting up a scale of, say, length. Temperature is not measurable directly. To establish a scale of temperature which does not suffer from the arbitrariness of Mohs' hardness scale, some measurable property which varies with temperature is needed. Such a property will be called a thermometric property. Length, volume, pressure, electrical resistance, and solubility are examples. But to establish a scale of length, a measurable property varying with length is not required. Length is thus directly measurable. A scale of length could, of course, be set up indirectly. Weight, for example, could serve as a criterion for length, and we could define the unit of length as that of a piece of, say, platinum wire manufactured in such and such a machine and having a weight of 1 gram. But no such quantitative criterion is necessary (or desirable) for the establishment of a scale of length. Whereas a criterion does appear to be necessary for temperatures.

This being the case, there are two problems involved in setting up a temperature scale which do not occur in setting up a scale of length: viz., which thermometric property we are to choose, and what functional relationship we are to suppose to hold between the measure of this property and temperature. The first is the problem of choosing a property to serve as a criterion for temperature, and the second is the problem of deciding how the thermometric property is to be related to temperature. Following Mach,[2] we will call these problems:

(1) the problem of choice of thermometric property, and
(2) the problem of choice of principle of correlation.[3]

A second point is that, while length is an additive property, temperature is not. There are physical counterparts of the arith-

[2] E. Mach, *Principien der Waermelehre*, Chapter 3.
[3] This problem of choice of principle of correlation is, of course, somewhat different from the problem we noted in connection with fundamental measurement.

metical operations of adding or subtracting length, but none for temperatures. Hence, in setting up a scale of temperature, there is no need to adjust it so that any operations with objects at temperatures t_1 and t_2 will produce an object whose temperature is $t_1 + t_2$ or $t_1 - t_2$. Nor, for that matter, need we adjust it so that any other mathematical operations on temperature numbers should have a physical interpretation. Temperature is not an additive property: it is not a 'multiplicative' one either. We are thus faced with quite a different set of problems in setting up a scale of temperatures. And since one of the objects of this paper is to discuss the significance of the numbers assigned to objects as a result of measuring operations, I now propose to consider what reasons there may be for choosing one thermometric property rather than another, or one principle of correlation rather than another.

The choice of thermometric property

Objects can be arranged in a crude order of temperature without instrumental aids. The basic ordering relationships are 'feels hotter than', 'feels as hot as' and 'feels colder than'. But the order given by these relationships is extended and refined by our knowledge of what makes a body hotter or colder, and of what effects heating or cooling has on bodies. Thus we can say: "Today must have been colder than yesterday—there was ice on the ground", or "The temperature of melting lead is less than the temperature of melting aluminium, because lead melts before aluminium".

But objects can be arranged in more or less the same order by any of a large number of groups of ordering relationships involving thermometric properties. The simplest class of such ordering relationships refers to some object T having some thermometric property p which varies with the temperature of objects A, B, C . . . when T is placed in some specifiable relationship to them. The objects A, B, C . . . may then be arranged in the order of p. If this order is more or less the same as the order given by heat sensation, then p of T may be described as *a criterion* for temperature. If p is a measurable property, then the measure or reading of p may be used to provide us with *a measure of temperature,* and the object T may be described as *a thermometer.*

The problem we are here concerned with is the problem of choice of p and of T. What reasons can we give for choosing one property p rather than another, or one object T having the property p rather than some other object S which also has this property? We feel, I think, that any measure of temperature which is thermometer-dependent, that is, depends on the particular

properties of some particular object, cannot, save accidentally, be of much significance. Even if it depends on the special properties of some particular substance or group of substances, we feel this. What we feel is needed is a universal property of matter as a thermometric property, and a thermometer-independent procedure for determining the temperature of an object. At any rate, I now propose to consider whether it is possible to find such a property or such a procedure.

Two empirical facts should first be noted. The first is the existence of reproducible heat states, that is, states of bodies which can be described without involving any ideas of temperature, and which always give the same reading on *any* thermometer no matter what thermometric property or substance it employs. An example of such a state is water at the boiling point. The second empirical fact is sometimes referred to as the zero'th Law of Thermodynamics. It is the fact that after sufficiently long contact away from heat sources or refrigerators, the thermometric properties of bodies cease to vary, and their equilibrium is not disturbed by subsequently switching them around. In other words, if A is in thermal equilibrium with B and B with C, then A will be in thermal equilibrium with C.

These two facts together provide us with thermometer-independent criteria of temperature equality. That is, because of these two facts we do not need to specify a thermometer in order to be able to say of two objects that they are *at the same temperature,* or of one object that its *temperature has remained the same.* Temperature equality, whether of co-existent or non-co-existent objects, is not thermometer-relative. We can say of two objects that they are equal in temperature and mean by this that they are in thermal equilibrium with each other. And we say that the temperature of any given object has remained the same, and mean by this that its thermometric properties have remained constant.

These two points can best be illustrated by considering the situation which would arise if the zero'th law were not true, or if there were no reproducible heat states. Consider first the situation that would arise if thermal equilibrium were not a transitive relationship. Suppose that three objects A, B and C are placed together thus: $\lceil A \mid B \mid C \rfloor$, and that under these conditions their thermometric properties do not vary. But suppose that on changing A and B round the equilibrium is disturbed. Now if B is chosen as a thermometer, we will have to say that A, B and C are all at the same temperature. Whereas if C is chosen we will have to say that at least A and B are at different temperatures. Therefore temperature equality under these cir-

cumstances must be thermometer-relative. And it would only make sense to speak of two objects being at the same temperature if the thermometer with respect to which they are said to be equal were specified.

The second point is that if there were no fixed and reproducible heat states, there would be no background of order against which the temperature of an object could be said to have changed or not to have changed. There might, indeed, be movements or changes within the temperature order, but only of one object with respect to another

There are, then, thermometer-independent criteria for temperature equality, and thermometer-independent descriptions of fixed heat states. But this does not mean that there are thermometer-independent procedures for assigning temperature numbers to objects. We can, of course, assign numbers to the fixed heat states to represent their relative position in the temperature order, 100 to boiling water, say, 0 to water at the freezing point, 50 to boiling alcohol and so on. But this is quite arbitrary. 212 would, for this purpose, do as well as 100, 75 as well as 50, and 10 as well as 0.

Now how is this arbitrariness to be avoided? I have suggested that the appearance of arbitrariness could be avoided if a thermometric property which is a universal property of matter could be chosen as a criterion. That is, if there were a thermometric property possessed by all substances which always had the same value in the same fixed heat state, then the measure of this property, or some function of it, could be taken as the measure of the temperature. And this would be the least arbitrary choice that we could make. Now in fact there is such a property. The efficiency of a perfectly reversible heat engine working between a heat source and heat sink which are in fixed heat states is independent of the nature of the working substance. And the thermodynamic scale which is based upon it is the least arbitrary that we have.

To show this, let us compare it with the centigrade mercury scale. Roughly speaking, the centigrade mercury scale is set up in the following way. A vacuum tube containing mercury is immersed in boiling water. When equilibrium has been established, a mark is made at the mercury surface and assigned the number 100. It is then immersed in freezing water, and when equilibrium has been established the new level is marked "0". The glass is then calibrated according to length (or, more accurately, volume). At a mark half-way between "0" and "100", the mark "50" is placed—and so on.

Now about this calibration we can ask the following questions:

1. Why choose mercury—why not alcohol? If you do choose alcohol, you will be led to assign different numbers to the same objects under the same conditions.
2. Why choose *volume* of mercury as a criterion for temperature? If you choose, say, electrical resistance, you will get another scale.

A satisfactory answer cannot be given to either of these questions. But in the case of the thermodynamic scale, neither of these questions arises. The first cannot be asked, since, no matter which substance is used as the working substance in a reversible heat engine, the scale will be the same. And the second does not arise since no other thermometric property which is a universal property of matter and which always has the same value at the same fixed point has yet been discovered.

So, to this extent, the thermodynamic scale is less arbitrary than the mercury scale, and for similar reasons I believe that it is less arbitrary than any other scale that has yet been proposed.

But this does not mean that the thermodynamic scale is not arbitrary in any way. There does, I think, remain an essential element of arbitrariness in the choice of *any* temperature scale based on indirect measurement. I do not mean, of course, that it is arbitrary in the choice of unit. All scales are arbitrary in this respect—direct as well as indirect ones. Even in the measurement of length by metres we arbitrarily assign the number 1 to an object kept in Paris. The number 10 would do just as well. What I mean is that there is a choice of functional relationship between the measure of our universal thermometric property and temperature. Shall it be linear, or logarithmic, or exponential, or what sort of relationship? This choice is what Mach called the choice of principle of correlation. And to this we must now turn.

The choice of principle of correlation

As the argument has been presented so far, it would appear that good reasons can be given for the acceptance of the thermodynamic scale of temperature rather than any other without its being necessary even to mention theories about the nature of heat and temperature. And this may be taken to suggest that such theoretical considerations have no rôle in the choice of temperature scale. But this suggestion would certainly be false. Theoretical considerations (as well as practical ones) led Black to adopt the mercury scale. Theoretical considerations were behind Dalton's rejection of it. And, finally, theoretical considerations gave strong support to the adoption of the gas scale. Historically, it is quite

false to suggest that theories about heat and temperature have played no part in the choice of temperature scale.

But though this may be so, we may still ask whether the acceptance of any theory about heat and temperature, together with the facts upon which it is based, necessarily involves the acceptance of a particular scale or type of scale. Reasons, even good reasons, need not be logically conclusive. We may ask whether theories of heat and temperature even give us any good reasons for adopting a particular kind of temperature scale. For the fact that they have been thought to do so does not imply that they do. It will be argued here that, while theoretical considerations may have some bearing on the choice of temperature scale, there yet remains an essential arbitrariness on which theories of heat and temperature have very little bearing. This arbitrariness lies in the choice of principle of correlation.

The principle of correlation, as it is here understood, is the functional relationship between the temperature number assigned to an object and the measure of its chosen thermometric property, i.e. the function f such that $\theta^\circ = f(p)$. The only necessary restrictions which must be placed upon f are that θ must be defined for every value which p may in fact assume, and the numerical order of the temperature numbers assigned to objects according to this principle must correspond to the temperature order. If these conditions are not fulfilled we would not be justified in calling the number θ assigned to an object a measure of its temperature.

The second of these points I take to be sufficiently clear. The first may be illustrated by the following example. Suppose we were to adopt the principle of correlation $\theta = \log (1 - 1_{ice})$ where 1 is the length of the mercury column in an ordinary mercury thermometer, 1_{ice} the length at the ice-point, θ is then defined in the field of real numbers for values of $1 > 1_{ice}$. But there would, of course, be conditions under which $1 < 1_{ice}$, and $1 = 1_{ice}$. According to this principle, then, when $1 = 1_{ice}$, $\theta = -\infty^\circ$. But what are we to say is the temperature of a certain mixture of ice and salt which gives a reading of $1 < 1_{ice}$? It must have a temperature, since it can be placed in a unique position in the temperature order. It would seem, then, that if we were to adopt this as a principle of correlation we would have to say that its temperature is less than $-\infty^\circ$. The only alternative is to reject the principle of correlation. And this course is certainly the one that would be taken.

This points to what I think is a necessary condition for describing a procedure for assigning numbers to objects as a procedure for measuring some quantitative property. It is that the

number sequence should not be exhausted either above or below before the sequence of objects arranged in the order of this property.

Granting these restrictions, there are nevertheless infinitely many possible principles of correlation between temperature and thermometric property. Historically most but not all of these have been linear in form. Amontons used a linear relationship of the form $\theta \alpha p$ where p is the pressure of air held at constant volume. Celsius and Fahrenheit calibrated their scales by taking equal increments in the volume of a sample of mercury to correspond to equal temperature intervals. But linearity was rejected by Dalton who took equal increments in volume to correspond to equal increments in heat capacity. He suggested that the relationship between volume and temperature was a logarithmic one.

The initial reasons for adopting a linear correlation principle were no doubt reasons of simplicity. Why assume a complicated relationship when a simpler one will do?

But Black at any rate considered that his calorimetric experiments confirmed the assumption of linearity, and Dalton, considering similar experimental results, concluded that the assumption of linearity must be false. It is clear that both Black and Dalton believed that good reasons other than reasons of simplicity could be given for choosing one principle of correlation rather than another. If they were right—if good reasons could be given for choosing one principle of correlation rather than another—then the whole problem of choice of thermometric property could perhaps be circumvented. For it would matter very little which thermometric property or substance were chosen in a standard thermometer, if this thermometer could be calibrated on the basis of, say, calorimetric mixing experiments.

But there is a fundamental weakness of all *theoretical* arguments for adopting one principle of correlation rather than another. It is, that the very difficulty which these arguments seek to overcome crops up again in the theory. The problem is the problem of choice of principle of correlation, *i.e.* of a functional relationship between thermometric property and temperature. A theory of temperature proceeds by saying what temperature is. On the Caloric Theory, temperature is a measure of the pressure of caloric. On the Kinetic Theory, temperature is a measure of the average kinetic energy of the molecules. But why should we make such assumptions? Only because they enable us to predict the experimental results or laws obtained by use of uniformly, that is, linearly calibrated instruments. If, for example, a logarithmically calibrated instrument had been used from the start, *i.e.* if $\theta \alpha \log V$ had been used as a principle of correlation in the 18th century,

then the empirically discovered gas law of Gay-Lussac would have been of the form $PV = Re^{\theta}$. To account for this law, it would then have been necessary to assume in the Kinetic Theory that temperature is the logarithm of the average kinetic energy of the molecules. And this could then have been taken to support the original logarithmic correlation principle.

The only argument which I can see against the adoption of a non-linear correlation principle is not a theoretical argument at all, but an argument from simplicity. It could be argued with some justification that $PV = R\theta$ is a much simpler mathematical form for the gas law than $PV = Re^{\theta}$, and that the results of calorimetric mixing experiments would be much easier to predict on the assumption of linearity than otherwise. But, although this may be so, there is yet no guarantee that a linear scale is the simplest possible temperature scale. Indeed, it seems to me that from the point of view of the mathematical simplicity of temperature laws, the substitution of $\frac{1}{\theta}$ for θ (the absolute temperature) would achieve some further simplification. It would mean that instead of the gas law, $PV = R\theta$, we should have: $PV\theta = \text{const}$, and instead of the Second Law of Thermodynamics as it applies to thermodynamically reversible processes, $\Sigma\frac{Q}{\theta} = 0$, we should have: $\Sigma Q\theta = 0$. At any rate, it is by no means evident that temperature laws assume their simplest mathematical form when a linear correlation principle is adopted. If they do, then it is only an historical accident. But whether or not they do is not a question which theory can decide.

Thus, whatever theoretical considerations we may take, there remains an essential arbitrariness in the choice of principle of correlation. Only reasons of simplicity can guide us in this choice.

But surely, it will be said, theories do determine at least some aspects of our temperature scale. Is there not a natural zero for temperature? And is this not a theoretical prediction? Yes, this is certainly true. But let us see what it means and what it does not mean. It does not mean that there is a lowest member of the sequence of possible heat states. Indeed, the Third Law of Thermodynamics suggests that this is not the case. It does not mean that there is, or might be, a heat state with which the association of the number zero is particularly appropriate in view of the special mathematical properties of zero. The proposition $n \pm 0 = n$ cannot be used to predict the result of any physical operation involving objects at temperatures n°A and

0°A. To say that there is a natural zero for temperature, therefore, cannot be quite like saying that there is a natural zero for, say, distance. What it does mean, however, is that the temperature in °C always appears in fundamental temperature laws together with a constant (+273), and that a simplification of the form of these laws can therefore be achieved by a change of variable, *i.e.*, a change to the absolute scale. It also means that on the absolute scale there is a lower limit to the temperature numbers which are significant, and this conveniently is the lower limit of the real positive numbers, *viz.* zero.

To suppose that there is an object whose temperature is less than 0°A is to make a supposition which can be true only if certain accepted temperature laws and theories are false. For instance, to substitute a negative number for t_0, the temperature of the sink (condenser) in a reversible heat engine, is to give a value for the efficiency of this engine greater than 100%. Moreover, a negative temperature number on the absolute scale would have no interpretation on the kinetic theory of temperature.

Theoretical considerations, then, do influence us in the determination of certain features of our temperature scale. For if on any arbitrarily chosen temperature scale there are theoretical limits to the temperature numbers which are significant, we prefer to change the principle of correlation so that these limits are reflected by the properties of the number system.

But even when this is done we must, as Mach has pointed out,[4] always be careful about drawing inferences from the properties of the number system. From the fact that the set of real positive numbers is everywhere dense it does not follow that the sequence of heat states is everywhere dense. From the fact that this set of numbers is bounded below, but unbounded above, it does not follow that the set of heat states is bounded below and unbounded above. This is an important point, and we need to be constantly reminded of it, not only in connection with temperature measurement, but in connection with other kinds of measurement as well. For consider the effect on our thinking of a change to a non-linear correlation principle, where the numbers assigned to objects according to this principle are related to absolute temperatures by a relationship of the form $T = k \log A$, where A is the absolute temperature, and T is the new temperature. Then 0°A would be represented on this new scale by $-\infty$° T. Now it seems to me that if we had been brought up to use such a scale, the Third Law of Thermodynamics would have been taken as self-evident. Of course, we cannot reach $-\infty$° T. Moreover, the

[4] E. Mach, *Principien der Waermelehre*, Chapter 3

question of whether there could be an object whose temperature is $<-\infty^\circ$ T would have been rejected as absurd. But, obviously, there is nothing self-evident about the Third Law, and it is not absurd to ask whether there could be an object which is colder than one at 0°A.

The psychological effect of adopting one principle of correlation rather than another is clearly illustrated by this example. And although, from a mathematical point of view, a change of principle of correlation amounts simply to a change of variable, which may or may not simplify the mathematical form of temperature laws, the whole effect of such a change is likely to be much greater than this. It is likely to change quite radically our thinking about the subject of temperature. And this is so because we easily and naturally think that the properties of the sequence of heat states is reflected in the properties of the numbers by which we designate them.

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In conclusion, it seems to me that the chief reasons for adopting one principle of correlation rather than another for a derived magnitude are reasons of simplicity—simplicity in the mathematical form of laws involving this quantity. And although this problem of choice of principle of correlation is somewhat different from the corresponding problem which occurs in fundamental measurement, it seems to me likely that similar considerations would apply. If our physics could be simplified by a change of length scale or mass scale, then these changes should be made even if it means abandoning additivity.

II. *Mixed Measurement*

No magnitude is directly and fundamentally measurable over the whole of its range. Inter-atomic distances are not fundamentally measurable. Nor are intergalactic distances. Sub-atomic masses are not directly measurable. Nor are the masses of the stars or planets. To measure such distances or masses we need to adopt a quantitative criterion, and a principle of correlation. It may, therefore, be wondered whether in these extreme ranges length and mass should be regarded as derived magnitudes, much like temperature. And, if so, whether the same warnings should apply.

There are, I think, certain differences between the logical problems of measurement in extreme ranges and those of derived measurement. But it is my view that these differences are only superficial, and that the measurement of very large or very small

distances or masses is logically very like the measurement of, say, temperature. If I am right in this, then it seems to me that the question of whether space is infinite in extent is logically very like the question of whether there is an upper or lower limit to temperature. It is ambiguous in the same sort of way, and it contains similar traps.

It is not, however, my intention to discuss specifically distance or mass measurement in extreme ranges. I propose instead to consider one kind of time measurement as an example of mixed measurement, and to apply my analysis to the problem of whether the universe does or does not have a beginning in time. But what I have to say will, I believe, be of general significance. Similar things could be said about whether space is infinitely subdivisible, or whether it is infinite in extent, or whether there are "atoms" of time interval.

By "mixed measurement" I understand a form of measurement which, as it were, straddles fundamental and derived measurement. Such measurement always depends upon the adoption and measurement of a quantitative criterion for the quantity p to be measured. But it also depends to some extent upon the direct and fundamental measurement of the quantity p. Just how this is so can best be seen by considering an example. And the following considerations concerning time measurement are meant to illustrate the features and to bring out the problems of that form of measurement which I have described as "mixed measurement".

The measurement of time

Time, like temperature, is a measurable quantity. Events can be arranged in a unique order of time (at any rate by a set of relatively stationary observers). We must enquire then what type of measurable quantity it is. Is it directly or indirectly measurable? There seems to me to be no clear answer to this question. It is true that if we are timing an event—a foot-race for example—we can, by starting a pendulum swinging as the race begins, count the number of repetitions in this event (*i.e.* the number of pendulum swings) necessary to produce equality in respect of time interval with the given event. We may judge, for example, that the time for the race was equal to the time for ten pendulum swings. And this suggests that time interval should be regarded as a directly and fundamentally measurable quantity—like length.

But there is a difficulty in this. For, once the race is over, we cannot 'go back' and time it in this way. Only present, not past events, can be timed directly. So it looks as if we ought to

say that while present time intervals are directly measurable, past time intervals are not. And, odd as it may sound to say this, I think that this is what we must say.

I will, therefore, distinguish between what I shall call 'dating' and 'timing' procedures. By a timing procedure, I will mean what is ordinarily meant by this term. And, according to ordinary usage, it makes no sense to speak of timing now a past event. I cannot intelligibly say that I am now timing the 100 metres final in the last Olympic Games. By the term 'dating procedure', however, I wish to include any procedure for *measuring* the time interval between two past events or between a past and a present event. This is a special use of these words, and it is not connected very closely with their ordinary usage.

First we should notice that most of the dating done by historians cannot properly speaking be described as measurement. In most matters, historians have to rely on the reports of witnesses, or upon reports of these reports. They use techniques of sifting them for consistency, of checking them off against one another, of building up a coherent picture of the course of events, and so on. But none of this is measurement. I cannot be said to have measured the length of a table if all I have done is to consider the reports of three people who claim to have measured the table, no matter what investigations I may make concerning the character or the circumstances of the individuals concerned. It does not matter how well founded my judgment as to the length of the table may be as a result of these considerations: I cannot be said to have measured its length unless I have myself carried out the appropriate measuring procedure. Similarly, I wish to argue, a historian who relies upon diaries, reports, reports of reports and so on in assigning a date to an occurrence, cannot be said to be measuring anything.

However, there is something which I wish to describe as the measurement of past or past-present time intervals. The geologist who takes the salinity of the sea as a criterion for the age of the solid earth may, by measuring salinity, measure the age of the solid earth. If, in a less simple way, he takes the quantities of radio-active substances in the earth's crust as a criterion for the age of the earth he is measuring the age of the earth by an alternative means. Similarly, the archaeologist may measure the age of something by employing radio-active carbon criteria. The cosmologist may measure what he calls "the age of the universe" by measuring the red shift of distant nebulae. A man may measure the age of a tree by counting the number of annular rings. And so examples may be multiplied.

In each case, something is accepted as a quantitative criterion

for the age of something—that is, for the length of a past-present time interval. In each case, a principle of correlation between this quantitative criterion and age must be adopted. In each case, age is determined by measuring this quantitative property and employing the adopted principle of correlation.

The measurement of age, therefore, seems to involve similar problems to the measurement of temperature. There is a problem of choice of temporal property, and a problem of choice of principle of correlation. And it seems that age, like temperature, must be regarded as an indirectly measurable quantity.

But there is at least one very striking difference. This difference lies in the existence of a sort of correspondence principle. That is, there must be a correspondence of the results of timing and dating. Otherwise we should say that one or the other is inaccurate. Suppose, for example, that a certain process has been timed. Then if, afterwards, the beginning of this process can be dated by a dating procedure, then the results of these two investigations must agree. If they do not agree, then we would say that a mistake must have been made—or, eliminating this, that the dating procedure or principle of correlation employed is inappropriate.

And so it may be argued that really neither of the above problems exists. For whichever quantitative property we choose as a criterion for age, an individual principle of correlation can be determined experimentally by using this 'correspondence principle'. That is, we can time the changes in this property, and by this means determine how the measure of this property varies with age. And further, it may be argued, this is just what we do. We *time* the changes in radio-activity of a given substance, extrapolate the curve, and obtain *empirically* a principle of correlation between degree of radio-activity and age of sample. There is nothing arbitrary about this principle of correlation. It is fixed and determinate. And hence, in age measurement, there is no problem of choice of principle of correlation.

This argument is, I believe, a sound one. Principles of correlation between temporal property and time interval can be established by direct timing procedures—*but only over very limited time ranges.* A principle of correlation is a mathematical conversion formula. In this case it must be of the form

$$A = f(p)$$

where A is the age of a given event-type, p is the measure of a temporal property, and f is a monotanic function. But the validity of this formula can only be established for very small values of A—at most a few thousand years. In the dating of very distant past events we must extrapolate this relationship; and

there arises in place of what I have called the problem of choice of principle of correlation a new problem which, for convenience, I shall call the problem of choice of principle of extrapolation. For all that is required of the extrapolated principle of correlation is that, for very small values of A, it merges into the principle

$$A = f(p).$$

Thus any principle of correlation

$$A = \phi(p)$$

which is such that, as $A \to 0$, $\phi \to f$ will do as an extrapolated principle of correlation.

Consider an analogy. Suppose that temperature were a directly measurable quantity between 0° and 0.1°C. It would then be the case that over this range of temperature any principle of correlation between a thermometric property and temperature could be checked by direct measurement. But it seems to me that it would be an utterly fantastic assumption to suppose that because a given principle of correlation can be shown to hold in this temperature range it would hold over the whole temperature range. It would indeed place some restrictions on the choice of extrapolated principle of correlation. But there would nevertheless remain a very wide area of choice.

Thus I maintain that in the discussion of such questions as the age of the earth, of the solar system, or of the universe, age must here be regarded as an indirectly measurable quantity—more like temperature than like length. Dating is not, of course, exactly like temperature measurement. The existence of the "correspondence" principle between timing and dating does make dating logically somewhat different from temperature measurement. But, at the same time, it is logically very different from fundamental measurement, and it seems to me to be less misleading to consider dating on analogy with derived measurement.

The age of the universe

To conclude this paper, I would like to make some remarks concerning the question of the age of the universe.

When we say "The temperature of this object is 400°C" just what do we mean? We mean that by following any of a number of well-specified temperature measuring procedures with sufficient care, anyone would be led to assign the number 400 to the object in question. When we say that there is a lower temperature limit at —273°C, what we mean is that, no matter which object is taken under whatever circumstances, we will never be led to assign a number to it < -273 provided that we follow a recognised temperature measuring procedure with sufficient care. It does not

matter for the moment what reasons we may have for saying such a thing, this is what we ordinarily mean.

There is, however, a second sense in which we may speak of a lower temperature limit. In this sense there exists a lower temperature limit if, and only if, an object can be found or produced which is such that no colder object can ever be found or produced. Mach, for example, thinks of the possibility of a lower temperature limit like this. He says: "Whether the sequence of heat states is bounded above or below can only be decided by experience. If one cannot find a body which is warmer or colder than a body of a certain temperature, only then is there a limit."[5] A little reflection will soon show us that these two senses are quite distinct from one another. There is no contradiction in saying that we can always produce an object which is colder than a given object, and at the same time that no matter which object is taken under whatever circumstances we will never be led to assign a temperature number to it which is less than, say, —273. That is, there may be a lower temperature limit in the first sense, but not in the second. Also, it is possible that there should be no lower temperature limit in the first sense, but nevertheless a lower temperature limit in the second. This would be the case where the coldest possible object is assigned the temperature number $-\infty$. If an object at absolute zero could be produced, and we used a logarithmic correlation principle, this would be true. Now I wish to argue that there is a similar distinction to be drawn between different senses of the question: "Has the universe a beginning in time?". In one sense it is the question: "Is there a maximum age for events?". That is: "Is there, for whatever reason, a lower limit to the number which can be assigned to an event as the date of its occurrence?". But in the second sense it is the question: "Is there an event before which no other events occurred?".

Now it seems to me that these two questions have always been taken together as though they were one and the same question. But there is, I think, no contradiction in saying "Yes" to one and "No" to the other. It is logically possible that the universe should have a beginning in time in one sense, but not in the other. That is, it is logically possible that there should be no theoretical limit to the number which may be assigned to an event as its age, but that nevertheless there should be a first event. And also it is logically possible that there should be a theoretical limit to the age numbers of events, and at the same time that there should be no first event.

[5] *Principien der Waermelehre*, Chapter 3.

This now seems to me so obvious that I wonder why it has not been commonly accepted. I think the answer must be that we all, naturally, think of time from the time-keeper's point of view. And dates are thought of as dependent on time-keeping. We thus fail to see that prehistoric dating depends very little on time-keeping. It depends upon the discovery of temporal properties and the adoption of a principle of correlation between temporal property and age—in almost, but not quite the same way, as temperature measurement depends upon the discovery of thermometric properties and the adoption of suitable principles of correlation. When we adopt the latter point of view, this ambiguity in the question "Has the universe a beginning in time?" becomes almost self-evident. When, however, we fail to adopt it, and adopt instead the point of view of a recording angel, the ambiguity mentioned becomes unintelligible. It becomes nonsense to suppose that the age of the universe is infinite but that there was nevertheless a first event; and nonsense to suppose that there was no first event but that the age of the universe is finite. For, from a God's-eye viewpoint, either there was a first event or there was not. If so, then the age of the universe cannot be infinite—for even God cannot count an infinite number of events. If not, then the age of the universe must be infinite.

But for whatever reasons this distinction may have been overlooked, it *has* been overlooked, and it is of the first importance. It is important, because once this distinction is recognised, it becomes clear that the question "Has the universe a beginning in time?" may be either of two distinct questions:

1. Is there a theoretical maximum to the age number which can be assigned to an event?
2. Was there an event before which no other events occurred?

The answer to the first question will depend upon an examination of our prehistoric dating techniques, upon the kinds of temporal properties employed, and upon the choice of extrapolated principle of correlation. The second question is, I think, in principle undecidable. Not even a recording angel, if there were such a being, and he were part of the universe, could decide this question.

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THE PROBLEM OF COUNTERFACTUALS*

By R. S. WALTERS

In most fields of investigation, some parts of the content of knowledge are expressed in the form of conditional statements that have been called counterfactual, contrary-to-fact, or (and this is sometimes taken to be a term of wider scope) subjunctive conditional statements. The following are examples of this kind of knowledge, although they are not exhaustive:

(a) Conjectures about possible historical developments, given as an initial condition some supposed historical event, incompatible with what actually was the case. Such conjectures, if true—and they must be true if they form part of the content of historical knowledge—"must be considered along with all other true and relevant opinions in any reasonable discussion" of certain actual historical developments or of policies proposed concerning certain actual events. One conditional of such a kind is, "If we had adopted a different policy towards Germany in the 1920's, the Second World War would not have occurred".[1]

(b) Conditionals derived from statements about dispositions. In order to understand the sense of any statement ascribing a dispositional predicate to a term and to agree that such a statement is true, we need to understand and agree to be true certain counterfactuals. This can be illustrated by considering an explicitly dispositional predicate, such as "soluble". If it is known that this piece of sugar is soluble, then it is known that, if this piece of sugar had been placed in water at any specific time, it would have dissolved. It would also be known that, if it were to be placed in water, it would dissolve. Conditional statements of the last kind are sometimes regarded as not counterfactual on the ground that the falsity or non-fulfilment of the antecedent is not presupposed in their being asserted. They do, however, raise similar problems to those of conditionals normally recognized as counterfactual and suggest, indeed, some questioning of the description of conditionals as counterfactual on the ground that their antecedents are in fact false or unfulfilled.[2] (I discuss this below, p. 33f.)

The example of a dispositional term is one of those which Ryle calls[3] "highly specific or determinate". Dispositional terms of a highly generic or determinable kind do not so readily sustain,

* I am indebted to Professors J. L. Mackie and J. B. Thornton for criticisms of an earlier draft.

[1] R. M. Chisholm: "The Contrary-to-Fact Conditional", *Mind*, LV, 1946. Reprinted in Feigl and Sellars: "Readings in Philosophical Analysis", N.Y., 1949, p. 483.

[2] Cf. R. M. Chisholm: op. cit., p. 487, and K. R. Popper: "The Logic of Scientific Discovery", London, 1959, p. 434.

[3] "The Concept of Mind", London, 1949, p. 118.

if they do sustain at all, the derivation of counterfactuals. This is merely to say that the explication of highly specific dispositional terms is different from, and simpler than, that of the highly generic ones. For the present purposes it is sufficient that, in some cases, knowing that a dispositional term is truly predicated of something involves assenting to certain counterfactual conditionals.

(c) Conditionals derived from scientific laws, particularly those claimed not to have existential import: that is, scientific laws which contain such terms as "bodies which are freely falling, or at absolute zero, or in a perfect vacuum".[4] In cases of this kind, although one may not expect to find, for example, bodies which are freely falling, certain counterfactuals of the kind, "If body X were freely falling, it would do Y", are nevertheless accepted as true. In fact, one of the things taken to be puzzling, but nevertheless of importance, in cases of this kind is that the subject terms of the laws in question are thought to be not merely states of affairs that have not occurred, but states of affairs that are empirically impossible. The laws are themselves equivalent to such statements as "If there were any freely falling bodies, then . . .", or "If there were any gases with molecules of zero-extension, then . . .",[5] when it is the case not merely that there are not such states of affairs but that there could not be such states of affairs. The impossibility here is not logical, but something less than this: the world is so constituted, it might be put, that there could not be such states of affairs.

In this respect, case (c) conditionals might be thought to raise problems different in kind from those raised by case (a) and (b) conditionals, because these latter do not raise issues of empirical impossibility. The states of affairs conveyed in their antecedents are empirically possible, although the antecedents are as a matter of fact false. Such states of affairs could have been naturally expected to occur, whereas the states of affairs conveyed by the antecedents of (c) could never actually be expected to occur.

I do not think that this is even true of all cases of propositions, described as natural laws, with subject terms that do not exist. If the proposition, "All ravens surviving in snowy regions are white", were regarded as a law and there were no such ravens, it would not be immediately plausible to suggest that it is empirically impossible for there to be such ravens. It might, however, given further knowledge, be held properly that their occurrence is empirically impossible. By this, I mean that

[4] R. M. Chisholm: op. cit., p 487.
[5] An example from R. B. Braithwaite: *Scientific Explanation*, Cambridge, 1953, p 298.

zoological investigations could discover some law or set of laws with which the proposition, "There are ravens surviving in snowy regions", is incompatible. This is to say that empirical impossibility, like other kinds of impossibility (and, one can add, possibility and necessity), is determined relatively to some set of propositions: in this case, of propositions accepted as laws.

It would, however, be granted that we have no such reasons now for believing it to be impossible for ravens to live in snowy regions; and if none do, then their not doing so is sheerly a matter of fact, empirically possible, but not actual. So far as the problem of counterfactuals is concerned, empirical impossibility raises no special problems, and case (c) is, in general, of the same kind as cases (a) and (b).

There may be a problem when, in the body of propositions making up a science, the subject term of a proposition accepted as a law is empirically impossible relative to some other proposition in that science also accepted as a law. Even this, however, does not seem to be a problem of any great difficulty.

Without going into the matter, I suggest that, when any proposition accepted as a law contains such a term, that proposition will be a derivative law in that science: it will be "not merely the generalization by itself, but . . . the generalization in the context of an established deductive system in which it appears as a deduction from higher level hypotheses which have been established independently of the generalization". This is a quotation from Braithwaite,[6] and although he is discussing here W. E. Johnson's well-known "brakeless train",[7] something which is empirically possible, his general comments are applicable to any case where a term is in some way empirically impossible. Whatever propositions concerning the danger of brakeless trains that we agree to be true are derived from higher-level hypotheses such as these: "that momentum increases with mass and velocity, that bodies with large momenta moving against only small retarding forces are dangerous" and so on. To this I should add that these propositions would still be true, even if there were also in the body of the science concerned a set of propositions, compatible with these "higher level hypotheses", that implied that no trains are (could be) brakeless.

The meaning of the term "counterfactual"

The terms, "counterfactual", "contrary-to-fact", and "unfulfilled", as qualifying "conditional statements", are normally thought of as equivalent. Conditionals so described have been thought to

[6] Op. cit., p. 305
[7] *Logic*, Part III, p. 12.

be counterfactual because the antecedent is counterfactual or false. It has also been supposed that a further peculiarity of such statements is that, when true, they express some "necessary connexion" between antecedent and consequent, independent of the truth or falsity of the antecedent and consequent. This is often taken as a causal or nomic necessity; but what I want to argue is that this "necessary connexion" is a concealed or assumed logical necessity, and that this introduces another possible meaning of "counterfactual". In its second sense, the term is co-extensive with what certain philosophers have called 'subjunctive conditional statements", where this has been assumed to apply to statements such as, "If X were to be Y, it would be Z". In this sense, it is wider in scope than it is in its first sense, since the antecedent need not be false.

The two senses of "counterfactual" are, then, (1) "factually false", and (2) "independent of fact". The second may be an unusual interpretation, but it is one that will bear investigation.[8]

(1) *Factually false.* In the case of contingent categorical propositions, the description of them as "counterfactual" is most appropriate when they are as a matter of fact false. The proposition, *p*, is counterfactual when not-*p* is the case. In the case of conditional propositions, this is not necessarily so. To say that a conditional proposition is counterfactual is not to say that it is false in fact: indeed, in most of the cases discussed the conditional is thought to be true and discussion has tried to discover what its being true could consist in. Thus "counterfactual" has not been used to mean "factually false conditional", but rather "conditional with a factually false antecedent".

(2) *Independent of fact.* There is a possible sense of the term, although I do not believe that it has been intended in this sense at any time, in which one could describe certain conditionals as counterfactual, without thereby committing oneself to any belief about the truth or falsity of the antecedent. In this sense "counterfactual" is taken as equivalent to "non-factual" or "other-than-factual". To describe a conditional statement as counterfactual in this sense, then, would be to say that it is a non-factual or other-than-factual statement, namely, that it is a formal implication.

The sort of conditional statement that is normally discussed as counterfactual is not, as it stands, a formal implication, but could only be so considered along with certain assumed premises. I think that it is possible to hold consistently that what are called "counterfactual conditionals" can be so considered, and that the

[8] Professor J. B. Thornton first suggested it to me.

"necessary connexion" of antecedent and consequent is but the concealed logical necessity of an argument.

An account of what it is for a counterfactual to be true or false

When it is said that some counterfactuals convey part of the content of knowledge, it is usually meant that what is conveyed is in some sense empirical or scientific knowledge. There are conditionals such as "If this had been a square, it would have been four-sided" and "If this had been a square, it would have been twelve-sided" that (given that the "this" in the second is not a twelve-sided figure) are straightforwardly logically necessary or logically impossible; but these are not thought to raise any serious problems. Only those that are thought, in some sense, to contribute to our understanding of the world are thought to raise problems.

These counterfactuals, since they convey part of the content of knowledge of the world, must be true; and, since, presumably, not all counterfactuals are true, it must be possible to indicate how those which are true differ from those which are false.

(I) *True as material conditionals*

If counterfactual conditionals were always what I have called conditionals with counterfactual antecedents, then the following statements would be true:

(i) any such conditional, taken as a material conditional, is true, since $p \supset q$ is true when either p is false or q is true;

(ii) the logical relation between any such conditional and another with the same antecedent but the contradictory of the consequent will be subcontrariety.

Consider the following pair of conditionals and assume that the common antecedent is as a matter of fact false:

(a) If ravens had survived in snowy regions, they would have been white;

(b) If ravens had survived in snowy regions, they would have been black.

Let (a) be written as "$A \supset C$" and (b) as "$A \supset$ not-C". Some restatement of the original antecedent and consequent in the indicative rather than the subjunctive mood will be necessary here, but, given this restatement, such conditionals will be true when either A is false or C true, and false only when A is true and C false. Given A as false, then $A \supset C$ and $A \supset$ not-C are both vacuously true.

Anyone troubled by conditionals of this kind with false antecedents may admit that, taken as material conditionals, they

are vacuously true and that a pair of the kind given are in subcontrary relation; nevertheless, he may still claim that such details as these are irrelevant to the questions that interest him.[9] The fact that A is false is of no consequence to him whatever. He wants to disregard this contingent fact and to establish the consequences of A's being true, whether or not A is in fact true. Whatever these consequences may be, it should be clear at once that the consequences of A's being true are not purely logical consequences of A, but the logical consequences of A together with certain other propositions implicitly assumed or not yet specified. "Ravens have survived in snowy regions", if this is regarded as A, has few logical consequences, and none of them could in any way determine whether or not those ravens surviving are white. This would have to be determined on quite other grounds. His problem is, then, one of showing which of C or not-C is the case, on a supposal that A is the case. This amounts to the problem of saying which of two corresponding conditionals of types (a) and (b) is true and which false. The assumption here is that they cannot both be true and if interpreted as material conditionals, they could both be true, then something other than this vacuous truth is required.

(II) *Another interpretation*

If a counterfactual is true or false, it is reasonable to expect that it will be either necessarily true or false (a matter of logical necessity) on the one hand, or true or false as a matter of fact, on the other. Counterfactuals of the kinds in question do not clearly seem to be one or the other. It seems foolish to suggest that, if true, they are necessarily true, and equally that they are true just as a matter of fact. Nevertheless I think that both of these notions are included in the truth of any counterfactual. The view that I am maintaining is that the claim that a counterfactual conditional is true is equivalent to two other claims:

(i) that a set (S) of contingent propositions is true as a matter of fact; and

(ii) that this set (S) together with the antecedent (A) entails the consequent (C).[10]

The claim that a given counterfactual is false is equivalent to a denial of (i) or (ii) or both.

Anyone asserting a counterfactual commits himself in these

[9] As W. M. Kneale claimed. See "Natural Laws and Contrary-to-Fact Conditionals", reprinted from *Analysis*, in *Philosophy & Analysis*, ed. M. Macdonald, p. 228.

[10] Nelson Goodman in *Fact, Fiction & Forecast*, London, 1954, p. 18, presents such a view, having discussed the criteria of the set S with great subtlety.

two ways. The conditional statements called counterfactual are elliptical statements, when not used truth-functionally, and the consideration of such conditionals will make explicit the contingent or factual assumptions or presuppositions made, and show whether or not the implicit inference that has been thus made explicit is valid or invalid. The determination of the truth or falsity of a particular conditional of such a kind will depend upon the discovery of relevant factual propositions, whatever these might be. The determination of whether a particular assertor *knows* that his counterfactual is true will depend upon whether he knows certain relevant factual truths. One fact which is relevant to whatever might be thought problematic about counterfactuals is this: that no conditional called "counterfactual" generates or implies the contextual assumptions or presuppositions which help to determine its truth or falsity.[11]

Any conditional, asserted counterfactually or subjunctively, is, I am claiming, an implicit inference. "If ravens survive in snowy regions, they are white" differs in meaning from "If ravens had survived in snowy regions, they would have been white" and "If ravens were to survive in snowy regions they would be white", in the way which "X must be Y" differs in meaning from "X is Y". Just as the modal "must" (if it is not, say, merely an emphatic way of asserting an A proposition) contains an implicit reference to an argument, and so to unstated premises, so a counterfactual conditional, because of its use of the subjunctive mood, contains an implicit reference to an argument and so to unstated premises. The meaning of a counterfactual is not clear until such premises are made explicit. The grounds upon which such a conditional as "If ravens survive in snowy regions they are white" might be asserted are irrelevant to its meaning. But in the case of conditionals of the kind "If ravens survive in snowy regions they must be white", and "If ravens had survived in snowy regions, they would have been white", the grounds are necessary to their meaning, because their meaning is in each case that of an argument or inference with unstated premises.

R. M. Chisholm argues [12] that the meaning of a counterfactual ought not to be confused with the grounds upon which it is asserted. Asserting, for example, "If A, then C" (counterfactually), we may not know the grounds of its truth, but be merely conjecturing that there is some set of true propositions, S_1, guaranteeing its truth. But we understand the meaning without

[11] Normally, when counterfactuals are spoken of, it is the conditional statements only that are referred to. I am suggesting that this is a loose use of the term and, strictly used, it should refer to the complete argument. I fear that I have not been consistent in adopting the strict use of the term.

[12] Op. cit., p. 490.

knowing anything about S_1. I do not think that this is the case, although it seems at first sight that what Chisholm is saying is intuitively right. The same kind of immediate appeal to intuition could be made about the relation holding between "If A, then C" and "If A, then not C" (both asserted counterfactually), and here it would suggest that these two statements could not be true together. This is not, however, supported by a logical analysis of possible cases.

It may be said that, in asserting 'If A, then not C", I am again conjecturing that there is a set of true propositions, S_2, which guarantees the truth of this conditional. In certain cases when A is false, it is the case that "If A, then C" is true (given S_1), and "If A, then not-C" is true (given S_2).[18] Suppose for example that both

(P) All ravens are black, and

(Q) All surviving in snowy regions are white,

are true; and, further, that it is false that ravens survive in snowy regions. If X asserts that, "If ravens had survived in snowy regions, they would have been black", and assumes (P), then his counterfactual is true. If Y asserts that, "If ravens had survived in snowy regions, they would have been white", and assumes (Q), then his counterfactual is true as well. This is a result surprising to our intuitions of the sense of these statements, only if in our intuitional understanding we neglect their character as truncated arguments. I think that the claim that we can understand "counterfactuals" independently of the grounds on which they are asserted and the claim that two counterfactuals of the kind mentioned are in contrary relation are variant statements of the same theory of counterfactuals, and a demonstration that two conditionals of the kind given may be true together is at the same time a demonstration that we do not understand the meaning of given "counterfactuals" without knowing the grounds on which they are asserted. We do frequently understand what people mean when they assert "If A, then C" (counterfactually) because we supply satisfactory premises ourselves which are the same as those assumed by the assertor. Otherwise we may misunderstand by assuming premises not assumed by him or we understand the statement as one awaiting completion, with, perhaps, some views of our own about possible ways in which it might be completed.

Consideration of an objection to this second account

The account that I have here given is in accord with the conditions set down of the truth of a counterfactual. X and Y

[18] Cf. K. R. Popper: "On Subjunctive Conditionals with Impossible Antecedents", *Mind*, LXVIII, 1959, pp 518-519.

may both be asserting true counterfactuals, but they can do so only if the common antecedent is false. There may, however, still be serious dissatisfaction expressed at this view. Someone may say that the view expressed is quite beside the point and the question has not been squarely faced. There is some sense, such a person may say, in which X and Y cannot both be right, even though their common antecedent is false: there must be something further that we can discover showing that one is right and the other wrong. What has to be considered is what follows, not when we know that A is false, but when we *suppose* that A is true. Even granted that counterfactuals are incomplete until the assumed premises are made explicit, one set of premises must be in some sense less true than the other.

Clearly, there is a problem here. In the case considered, since the consequents of X's and Y's conditionals are incompatible, the supposal that A is true means that either (P) or (Q), the propositions presupposed by each, would have to be false. Formally the position is this: the following set of propositions comprises the premises from which X and Y deduce their incompatible consequents:

(P) All ravens are black;

(Q) All surviving in snowy regions are white; and

(A) There are ravens surviving in snowy regions.

This set forms an inconsistent triad,[14] so, given that (A) is the case, one of (P) or (Q) must be false.

A precisely similar kind of situation exists with all counterfactuals. The examples, "If Hume and Voltaire were compatriots, then Hume would be French", and "If Hume and Voltaire were compatriots, then Voltaire would be Scottish", could both be true, given that X, asserting the first, assumes "Voltaire is French", and Y, asserting the second, assumes "Hume is Scottish". In this case the combined premises again form an inconsistent triad:

(P_1) Voltaire is French;

(Q_1) Hume is Scottish: and

(A_1) Hume and Voltaire are compatriots.

The inconsistent sets of propositions derivable from pairs of counterfactuals similar to those given would be in many cases more complex.

If one were told to assume that (A_1) is true, and then to determine which of (P_1) or (Q_1) would have to be false, consternation would be justified. (P_1) and (Q_1) do not lend them-

[14] This point and the substance of the argument arising from it were suggested by Professor J. L. Mackie.

selves to determinations of this kind. Assuming (A_1) does not provide an occasion for rationally deciding between (P_1) and (Q_1). On the other hand, most philosophers would agree that the similar question asked about the set (P) (Q) (A) is not so clearly undecidable. Assuming (A) does provide an occasion for rationally deciding between (P) and (Q), and the attempt made by some philosophers to distinguish nomic universals or law-like generalisations from accidental generalisations is an attempt to enable the specification of those universal propositions in inconsistent sets such as (P) (Q) (A), which would have to be true or have to be false if propositions of the kind (A) were true. In most, if not in all, cases in which it is regarded as a sensible undertaking to decide, in a given inconsistent set of propositions, which must be false if the antecedent common to two counterfactuals is supposed true, the issue is likely to be between a supposedly law-like and a supposedly accidental generalisation.

In the example given, there would be little hesitation in saying that, given (A) as true, then (P) is false, but the reasons for this do not consist in any formal difference in the kinds of universal that they are, but rather in the relations which they have to other propositions in the body of science.

Law-like and accidental universal propositions

The discussion concerning law-like and accidental universal propositions, where this distinction has been supported, has normally sought for some criterion by means of which these two may be distinguished.[15] One criterion advanced is that law-like propositions sustain counterfactuals whereas accidental ones do not. "All surviving in snowy regions are white" sustains the counterfactual, "If X (whatever X might be) were to survive in snowy regions, X would be white". On the other hand, it is said, a proposition of the kind "All my friends speak French" will not sustain the counterfactual, "If X (whoever X might be) were one of my friends, he would speak French".[16] But this will not do as a criterion for distinguishing the supposedly different kinds of proposition, since on one view of what "sustains" might mean, any true universal proposition sustains a counterfactual, in the sense that the counterfactual so derived is in each case true, in accordance with the conditions set down above. Thus, I should argue, these two propositions could sustain counterfactuals in this sense. If it is urged against this that one sustains a counterfactual

[15] E.g., W. M. Kneale: *Probability and Induction*, Oxford, 1949, p 75. See also "Natural Laws and Contrary-to-Fact Conditionals", reprinted from *Analysis* in *Philosophy and Analysis*, ed. M. Macdonald, p. 228

[16] A Popper example from "A Note on Natural Laws and So-called 'Contrary-to-Fact' Conditionals", *Mind*, LVII, 1949, p 64

and the other does not, by which it is meant that one counterfactual is true, whereas the other is not, then the "true" counterfactual must be true in some stronger sense than I can allow. Granting for the sake of argument that there might be such a strong sense of "true", I can only say that the difference in kind between two universals would have to be known before it could be said whether or not one of them "sustains" a counterfactual in this sense, and thus the sustaining of counterfactuals could not be used as a criterion for distinguishing law-like universals from accidental ones. My own argument, however, is such that I cannot grant that there might be such a strong sense of "true".

On the account that I have given, both universals would "sustain" counterfactuals, since the process of sustaining counterfactuals is similar to the process of adding determinants to the subject term of universal propositions. "All X are Y" entails that "All X that are A are Y", whatever A might be. This is so if the subject term, X, is an unrestricted descriptive term, numerically neutral or of indefinite extension. If "my friends" were such a term, then the universal, "All my friends speak French" would sustain the counterfactual "If Confucius were one of my friends, then he would speak French". I should say that it does sustain the counterfactual and that any unrestricted universal proposition sustains counterfactuals. The dissatisfaction that is felt with this result is not a dissatisfaction with the logical procedure, but arises because incidental factual knowledge is brought into consideration. The fact that Confucius does not speak French is introduced into the logical context, providing another set of inconsistent propositions:

(a) All my friends speak French;

(b) Confucius is one of my friends; and

(c) Confucius does not speak French.

When it is said that a proposition such as (a) does not sustain counterfactuals, the error involved seems to be this: that (b), the antecedent, assuming it to be true, is thought to falsify (a), the proposition under consideration. It falsifies (a), however, only when conjoined with a proposition such as (c). If (c) is not explicitly given, then (b) would falsify (a) only if it implied or "presupposed" (c). It does not imply (c), since (b) and (c) are indifferent to each other, and it would not be plausible in most contexts to claim that (b)'s being asserted presupposed (c).

I take these arguments to be sufficient to dispose of any attempt to suggest that sustaining counterfactuals or making counterfactuals true in some strong sense is a way of distinguishing law-like from accidental universals. If there is to be such a

distinction of universals, then it must be made on grounds different from these. The possible grounds for arguing that some universal propositions are accidental and some are law-like seem to be two, and they are not necessarily appealed to together:

(1) The first is, that accidental universals are such that their subject term contains some implicit restriction or limitation of the numbers denoted. This view is that such propositions are the results of summative inductions. Their subject terms are not unrestricted descriptive terms nor are they numerically neutral and of indefinite extension. In this sense, it may be claimed that "All my friends speak French" contains some kind of implicit restriction of the number of my friends: in other words, that the proposition itself is equivalent to a finite conjunction of singular propositions of the form, "X is my friend and he speaks French". The proposition is not explicitly so restricted, and it would need to be *shown* that it is implicitly so. There is no sense in calling such a proposition "accidental", unless this kind of restriction can be shown, or unless the following second sense is intended.

(2) The second is that those universals are accidental for which, although the subject term is an unrestricted descriptive term, and so number-neutral, no reason can be found in a universal proposition of wider scope.[17] In other words, the universal in question cannot be deduced from a universal proposition of wider scope, together with certain other propositions. On this view, "All ravens are black", although its terms are unrestricted descriptive terms, does not follow from any wider principle or universal. Indeed, in this case, a universal of wider scope (some expression of the principle of natural selection) apparently justifies or provides a reason for a contradictory of that proposition, although that contradictory may not be true in fact.

When, as here, two true propositions are thought of as in some sense incompatible, it can only be by considering them together with some other proposition that is false as a matter of fact but is such that, if it were true, then one or other of the original propositions must be false. In fact, it could be said generally that, for any categorical proposition, *p*, which is as a matter of fact false, at least two other propositions, *q* and *r*, could be found such that the set *p q r* is an inconsistent triad. Thus, in such a set, if *p* is supposed true, then either *q* or *r* is false. In some cases, as suggested, no rational grounds for preferring one of these rather than the other could be given. In some other cases, there would be rational grounds for preferring one propo-

[17] This clearly will not do for any laws claimed to be ultimate. Their non-accidental character might be established by the number and variety of laws that they justify Cf R. B. Braithwaite· *op. cit*, p. 300 ff.

sition rather than the other. Where *q* and *r* are universal propositions, the proposition rejected may be described as accidental. Such a description of it would accord with the second account given of this term, and would be a way of referring to its comparative isolation from the network of propositions in a given science or field of investigation. In contrasting it with another universal proposition from the same field said to be law-like, one would simply be drawing attention to its narrower range of crucial logical relations with the propositions of the science of that field. In other words, one would simply mean that, should such a proposition be found to be false, fewer revisions of the body of the science would be necessary than if one of those claimed to be law-like was found to be false. Any account of the sense of "accidental" and "law-like" stronger than this, for example one which sets up two grades of being universal, will not do.[18]

A decision concerning propositions in an inconsistent set such as the set *p q r* or (A) (B) (C) above will always be made, if it is made rationally, on the basis of preferring that proposition most strongly supported by the body of propositions in a science: in other words, that proposition which, if false, would require the more radical revision of that science.

Decidable and undecidable counterfactuals

The occasions for the use of conditional statements normally regarded as counterfactual are numerous, but the assent accorded to such statements can only be accounted for as the assent accorded to presupposed contextual propositions which are thought to entail the consequent as suggested. This assumes some common ground between assertor and hearer, some agreement that such and such are the contextual propositions. This assumption is sometimes warranted and sometimes not; but in any case of doubt there is scope for explicit statement of the contextual propositions. It is not, I maintain, until these contextual propositions are stated that the counterfactual is complete and the question of decidability arises.

"If X had been Y, it would have been Z", as a form of statement, is an incomplete counterfactual, and is similar to enthymeme and other arguments with suppressed premises. Given an enthymeme of the form, "A is B; therefore, A is C", we are provided with an incomplete argument which allows various possibilities of completion or interpretation; that is, various possi-

[18] Cf. K. R. Popper: *The Logic of Scientific Discovery*, London, 1959, pp 427-430, and R. B. Braithwaite, *op. cit.*, p. 295. A discussion of this matter with which I agree is given in R. Brown and J. Watling's: "Counterfactual Conditionals", *Mind*, LXI, 1952, pp. 226-231. Their general account of counterfactuals, however, is different from mine

bilities of supplying premises that result in a valid or invalid argument. It may, of course, be complete as it stands (in which case it would not be an enthymeme) and so be formally invalid. Until further information is given, namely, that the argument is complete as it stands, or that such and such premises are "understood", the argument is undetermined as valid or invalid.

The use of the subjunctive mood in a conditional statement indicates that such statements are elliptical arguments. Anyone asserting such a statement implicitly conveys to the hearer a guarantee that he can provide a set of contextual propositions which establish his counterfactual as true in the sense already suggested. This guarantee is often spurious and the statement merely the expression of partiality or of a wish: the assertor has in mind no set of contextual propositions, or the propositions which he has in mind are some justification for the consequent, but they do not, together with the antecedent, entail the consequent. Such counterfactuals (and by this I mean the completed statement) are decidable, and would, on my account, be decided as false.

Mr. Hampshire, however, has claimed[19] that many counterfactuals are in principle undecidable, while many others are in principle decidable. He makes this distinction in terms of the content of a subjunctive conditional statement, and the distinction is one which coincides with the distinction between non-scientific and scientific fields. In the non-scientific field, such as law, history or morals, subjunctive conditionals are used which are in principle undecidable, which express "judgments or interpretations of the facts".[20] In the scientific field, since "the language of science (by definition) consists wholly of decidable statements", subjunctive conditionals are decidable as true or false. One consequence of this is that the question of whether borderline or disputed enterprises are scientific or not can be answered by determining how their subjunctive conditionals are to be considered.[21]

On the face of it, there seems to be some case for saying that conditionals such as, "If this metal had been heated, it would have expanded" are in principle decidable; and there seems to be no equally strong case for saying that conditionals such as, "If Hitler had invaded in 1940, London would have fallen" are in principle decidable. This is, however, misleading: each statement is, as it stands, incomplete and, according to the view I have

[19] "Subjunctive Conditionals", reprinted from *Analysis* in *Philosophy and Analysis*, ed. M. Macdonald, Oxford, 1954, pp. 207-3.
[20] *Ibid*, p. 210.
[21] *Ibid.*, p. 208.

put forward, the question of decidability does not arise until the statement is completed. In the examples given, we should normally complete the first by the universal proposition, "All metals expand when heated", and, depending upon whether or not we regard this as true, we decide the counterfactual as true or false. We would, however, normally have no regular way of completing a statement of the second kind, and would await completion in whatever way it might be given. Once the statement is completed, I can see no reason why it could not be decided as true, in accordance with the conditions specified.

According to Hampshire[22] lawyers do not describe a counterfactual in a judgement or a legal argument as true or false: if they disagree with it, they dispute the "interpretation" of the facts which it conveys. Similarly, historians and moralists may disagree with the interpretations of facts conveyed in a counterfactual. In my view, however, they might, on certain occasions, be justified in saying that the counterfactual in question is false, or they might allow the counterfactual to be true, asserting another of their own which, while apparently contrary to the original, is true, in the way in which X and Y above asserted true counterfactuals in their apparently contrary claims. The natural place of the conditional statements in question in a historical exposition or a legal judgement is either at the beginning, where they anticipate a body of facts, however these might be selected, already "worked through", as it were, or at the end, where they succeed it as a summary statement.[23] Given this body of facts, as I say it should be given, as part of a counterfactual, then it makes perfect sense to say that this counterfactual is decidable. A counterfactual is only undecidable when for some reason its complete form is undiscoverable, or (and in this sense it might be claimed that counterfactuals incorporating natural laws are undecidable) when some constituent factual propositions are undecidable.

It might be claimed that, while all counterfactuals are excursions into logically possible worlds, nevertheless historical and legal counterfactuals make far greater excursions than those containing, for example, a single natural law and a statement of initial conditions, such as "If this metal had been heated, it would have expanded". This might be urged as a reason in support of their undecidability in principle. This reason, however, will not do.

It might be said, for example, that a judge who asserts that if Jones had not pulled the emergency cord, the accident would not have happened (and this is equivalent to his asserting that Jones's

[22] Op. cit., p 208.
[23] Cf. Hampshire. *op. cit.*, p. 208, and P. Gardiner: *The Nature of Historical Explanation*, O.U P., 1952, p. 90.

pulling the cord is a necessary condition of the accident), or a historian who asserts that if Hitler had invaded in 1940, London would have fallen (and this is equivalent to his asserting that Hitler's invading is a sufficient condition of this state of affairs) could only defend their assertions by arbitrarily postulating certain conditions, by arbitrarily taking certain conditions as given. Given these arbitrary limiting conditions, it might be said, then the conditional could be decided but not otherwise: it is only in this artificial way that one can, in historical or legal matters, determine what would have happened as a consequence of a state of affairs which is contrary to fact.

These cases are, however, only different in degree and not in kind from the so-called decidable cases. When one claims that if this metal had been heated, it would have expanded, one assumes in addition to the universal proposition, a proviso of an other-things-being-equal kind. This proviso is a perfectly general one in such a case, but it could be made explicit in numerous ways. When, in the case of a historical or legal counterfactual, certain conditions are postulated as fixed or constant, this is, in my view, equivalent to making explicit what might be conveyed by the other-things-being-equal proviso. If someone wants to know what *really* would have happened, if a given actual event had not occurred, there is, of course, no reply that he can be given until we are told what precisely he wants to include in his possible world. What occurs in a possible world depends upon what one puts into that possible world, upon what one takes as fixed or constant in it, and considerable variety is possible in what one takes as constant.[24] Any belief that there must be something which is *really* the case when one makes a contrary to fact supposal, is based on the same grounds as the desire for a strong sense of the truth of counterfactuals, and according to my view there is no such strong sense.

Given that Voltaire is French and that Hume and Voltaire are compatriots, it is "really" the case in this possible world that Hume is French. Given that all ravens are black and that there are ravens surviving in snowy regions, then it is "really" the case in that possible world that those ravens are black. The counterfactuals of law and history are in my view of a precisely similar kind as these, their decidability, or perhaps more strictly, their being true, depending upon which true propositions particular assertors incorporate in their possible worlds. Such counterfactuals may be slightly more complex in their constituents than

[24] When it is said that a lawyer disputes the judge's interpretation of the facts, it is this selection of what is fixed or constant that is disputed or objected to.

some others, although this need not be so; but the general conditions under which such counterfactuals are true are the same as those under which any counterfactuals are true.

Conclusion

The outcome of my discussion is this: that, since the truth of counterfactuals, properly interpreted, is an amalgamation of factual truth and necessary truth (or validity of argument), then counterfactuals are of little importance in any field of knowledge. Certainly, they do not in any way advance knowledge or express mysterious more-than-factual connexions of facts: they only convey part of the content of knowledge by incorporating in their complex form, normally implicitly, matters of fact already known or assumed to be true. When counterfactuals with incompatible antecedents and incompatible consequents are considered, it is not the case that one expresses a merely accidental connexion of states of affairs and the other an essential or more-than-factual connexion. Whenever a decision is made between them, what is being decided is which of an incompatible set of propositions would be accepted as true, if the false antecedent were true. In some cases, as suggested, no rational decision could be made one way or the other. In other cases, the grounds upon which a decision is made are grounds of least logical disturbance to the propositions in the body of a science. This is a matter of the logical relations holding among the propositions of a science, and is best discussed without referring to the so-called problem of counterfactuals at all.

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ON PHILOSOPHICAL ANTHROPOLOGY

By H. O. PAPPE

'On the continent of Europe', says Herskovits in his textbook, the term . . . 'anthropology is reserved for the study of physical type: cultural anthropology is not called anthropology at all'.[1] Yet if one surveys German and other Continental writing over the last thirty years, one finds a large number of books and articles dealing with anthropology in a sense quite different from that mentioned by Herskovits. These writers call their work 'Philosophical Anthropology' (PA hereafter). Far from accepting Herskovits' definition, many of them dispute the right of physico-ethnological anthropologists to use the term 'anthropology' to designate their work. According to them the original meaning of the term has, since the late sixteenth century, served to describe the basic knowledge of human nature and of the human condition. Conventional anthropology, in its search for details regarding man and societies, presupposes knowledge of what man really is. PA, however, questions this knowledge; it takes it as its very problem. It aims to elucidate the basic qualities and conditions which make man what he is and which distinguish him from all other beings. PA could then appear to have a well-defined task setting it off from other disciplines. However, it tends to operate as a co-ordinating system, which incorporates knowledge about man as evolved by other branches of the natural and social sciences. It thus aims to provide the master key for the understanding of man's position in the world.[2] One is reminded of the 'science of human nature' which Scottish and English philosophers have pursued since Hobbes, Locke and Hume; in particular of Hume's statement that 'in pretending to explain the principles of human nature we in effect propose a complete system of the sciences'.[3] However, as I shall endeavour to show, there are basic differences between this approach and the methods applied by many philosophical anthropologists.

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[1] M. L. Herskovits, *Cultural Anthropology*, 1955, p. 8.

[2] Kant distinguished physiological and pragmatic anthropology. While the former treats of man's limitations as set by nature, the latter deals with man's potentialities, with 'what He, as a free agent, makes of himself, or is able and ought to make of himself'. *Anthropologie*, Vorrede, p. 1. (In *Die Metaphysik der Sitten*, part 2, introd. III, however, Kant contrasts empirical 'Naturlehre' (Anthropologie) with a priori morals (Sittenlehre).) According to H. Plessner, PA is to mediate between and co-ordinate physiological and pragmatic anthropology. *Zwischen Philosophie und Gesellschaft*, 1953, p. 121.

[3] *A Treatise of Human Nature*. Introduction (Everyman ed. Vol. I, p. 5).

I

There is little to be found in English writing about PA, though Cassirer's *Essay on Man* belongs in its compass, and Collingwood's and Toynbee's thought shares important elements with it. On the Continent it has become a powerful movement and is, in fact, widely recognized as epitomizing the essential task of philosophy today. A look at concrete examples of PA will contribute to a better understanding of what it means. What sort of people actually do practise PA? Comparatively few are professionally in this line; there are only a few academic chairs of PA.[4] On the other hand, the number of professed adepts is surprisingly large, and so is their range over adjoining disciplines.[5] They are by no means restricted to Germany. There are many Swiss, Dutch, and French scholars amongst them. There are philosophers who attach primary or, at least in some way, fundamental significance to PA, such as Scheler, Plessner, Hartmann, Landsberg, Häberlin, Dempf. There are economists (Sombart), ethnologists (Frobenius, Jensen), historians (Ed. Meyer, Dilthey, Spengler, Rothacker, Litt), historians of art (Sedlmayr), sociologists (v. Wiese, Adorno, Gehlen, Freyer), theologians (Bultmann, Kuhlmann, Gogarten, Brunner), and there are biologists (Uexküll), zoologists (Portman), doctors (Weizsäcker), and psychologists and psychiatrists (Jaspers, Pfänder, Binswanger).[6] This is certainly not a complete list, but even so we are confronted with a baffling variety of research. We are not faced, as we might have expected, with the gradual growth of a new branch of science. It is rather that many scholars, well established in their own lines, found themselves within the pale of PA after its significance had been disclosed to them. We may think of Molière's bourgeois gentilhomme who was startled and proud to discover himself as speaking prose. It is natural to ask

[4] E.g. at Gottingen and at Nijmegen (the latter being a chair of psychology, anthropology, and pedagogics).

[5] Many philosophical anthropologists prefer to emphasize their peculiar perspectives in speaking of theological, historical, biological, phenomenological anthropology and culture-anthropology rather than of PA (which is, however, understood to permeate and co-ordinate these sub-divisions). Rothacker's Kultur-Anthropologie is not the same as American cultural anthropology though there are common traits.

[6] For an English language account see H. Wein, 'Trends in Philosophical Anthropology and Cultural Anthropology in Post-war Germany', 24 *Philosophy of Science* (1957), pp. 46-56; also David Bidney, *Theoretical Anthropology*, 1953, passim. A perceptive account of Kant's, Feuerbach's, Nietzsche's, Scheler's and Heidegger's anthropological theories is contained in part V of Martin Buber's *Between Man and Man*, 1947. Also Herder and Wilhelm von Humboldt belong to the 'founders'. Amongst the large number of historical and systematic contributions to the subject, Michael Landmann's textbook *Philosophische Anthropologie*, 1955, offers the most judicious and sober introduction to PA.

what can be the common denominator of this *embarras de richesse*?

When, in the English-speaking world, we nowadays speak of the modern 'revolution in philosophy', we think of philosophy having become more secular and more technical, largely preoccupied with the nature of philosophical enquiry and with the notion of meaning. On the Continent and in Germany in particular, philosophers like to speak of 'Copernican turns' rather than of revolutions, and, since the twenties, there has been a fashion of talking of the 'anthropological turn' in philosophy. Philosophical anthropologists set out from the conviction that the theory of knowledge had reached a desperate crisis. Traditional theory of knowledge is seen by them as occupied only with one of the functions of consciousness;[7] and consciousness, in turn, is understood to present only a part of the forces shaping human reason (as distinguished, in the Kantian sense, from understanding). To PA knowledge is not autonomous but depends on dispositions and heterogeneous influences. 'The idea of logic gets dissolved in the maelstrom of basic questioning', as Heidegger put it in somewhat extreme terms. This type of argument is used by the vitalist and existentialist schools of anthropological thought. PA, furthermore, is opposed to leaving the investigation of the empirical world solely in the hands of the scientists. 'Toward the things themselves', and the 'turn towards the objective' are expressions which mark the phenomenological attempt at developing a peculiar method for analysing or, rather, interpreting facts, qualities, relationships, and the basic categories of human nature and culture. This objective approach, to be sure, does not imply the application of the methods used by the empirical physical sciences; it refers to the peculiar philosophical methods which should be applied, in particular the phenomenological method.

PA thus aims to overcome what its followers call the 'crisis of science'. The crisis of science is said to have been brought into view by the modern developments in biology and psychology, and by the establishment of the indeterminacy principle in nuclear physics. It is difficult to see why these, or any, scientific advances should have been considered as constituting a crisis in science. However, representative German philosophers since Kant had long maintained a conception of science as something fixed on the lines of a rigid mathematico-mechanical determinism. This

[7] I.e. as leaving out of account what Pascal called the 'logique du coeur' or the 'esprit de finesse'. Hamann's, and especially Herder's philosophy, which rejected the 'vis rationalis' (the syllogistic reasoning faculty) as something partial and inadequate, is here a significant source.

basic conception, they feel, has broken down. There is rather wide consensus among Continental thinkers that the materialism of the nineteenth century has been overcome, and that the peculiar methods of the 'Geisteswissenschaften' have finally been vindicated. These methods are concerned with the significance imminent in events and in the works of men rather than with the causal nexus between events. Their aim is the interpretation of other minds (both individual and collective), of their peculiar motivations and tendencies as well as of the institutions in which their ideas have found expression. This 'hermeneutic' process is primarily directed towards the conscious and unconscious actions of human beings and towards the structure of interpersonal (social and cultural) relationships. It is conceived of as contrasted with the methodology of natural science. It is said to be descriptive-interpretative, organic, and concrete rather than explanatory, mechanical, and abstract. In short, instead of one logic or one methodology with a variety of rules, there is here a dichotomy resulting in two distinct methodologies, namely causal explanation and 'Verstehen'.[8] This attitude is not exactly novel. It rather takes up the theme of an older tradition in Continental philosophy, known in English thought as the Germano-Coleridgean school, with its emphasis, in Mill's words, on a philosophy of society in the form of a philosophy of history, and aiming at a philosophy of human culture.

The outstanding instance of the procedure favoured by PA is the phenomenological method which has been adopted by about half of all philosophers and an ever-increasing number of social theorists.[9] Rather than treating this procedure in detail I shall attempt to illustrate its operation by means of three examples of anthropological investigation. This should furnish a better taste of the dish, and it is also in keeping with the phenomenological method. I shall, however, say a few words about this method when we come to our third illustration.

My first example deals with the image of man in our own consciousness and why it should matter to us. I shall then review the meaning of technology in an anthropological perspective.

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[8] I am over-emphasizing the contrast for argument's sake. There is some common ground between linguistic philosophy and Continental trends For a recent example of British philosophy in a Continental key see P. Winch, *The Idea of a Social Science,* 1958. For an example of a detached treatment of the problem, taking into account both cis- and trans-Channel thought, see A. Carr-Saunders, *Natural Science and Social Science,* Eleanor Rathbone Memorial Lecture, 1958.

[9] I. M. Bochenski, *Die Zeitgenossischen Denkmethoden,* 1954, p. 25. It may be appropriate here to recall the names of Brentano, Meinong, Husserl, all of whom have, more or less, left some mark also on British philosophical thought.

Finally I shall turn to psychology to show how applied anthropological method actually works.

II

The self-image of man has become of central importance for various reasons. Science has permitted man to control nature with the following effect: Formerly man was up against man but primarily against nature in the form of wild animals, disease, hunger, cold, flood, poor soil, and so on. Today, nearly all these phenomena have been or can be brought under human control. Man is not threatened by his old enemy or by the God who made nature. He fears the man-made use of nature in the form of hydrogen bombs, etc. He is up against man, man-made structures or the God who made man. Finally even in the very picture which man (in his scientific representatives) has of nature today he meets himself rather than natural data. For the object of natural science is not nature as such any more, but nature as *we* question it for specific purposes and in the specific contexts of axiomatic frameworks determined by ourselves. The mathematical formulae which scientists use do not represent nature but rather our knowledge of nature. Man finds himself inescapably confronted by man.[10] We have every reason to ask this crucial question: what is this man? What, however, causes us to ask the question: in what form does man's subjective image of himself appear in his consciousness?

The self-image of man is understood by PA to be tremendously important. It determines what man makes of himself, into what sort of man he turns himself. Animals are as nature has created them. Man, however, is unique in that he must complete his character, for which nature has supplied only rudiments. He has to form his personality, and he does so according to the images in his mind of what he can and should be.[11] Seen in historical perspective man's consciousness of himself offers a pattern of five types, unfolding in progressive growth: from cosmological all-unity of animal and plant world, over the demarcation of man as the possessor of the Logos, as the child of God, to the pantheistic position of the late middle ages and of modern

[10] See W. Heisenberg, *Das Weltbild der heutigen Physik*, 1955, passim.

[11] I am using for this part an investigation by Scheler, which is considered to be one of the pioneer pieces of the anthropological discipline. M. Scheler, *Mensch und Geschichte*, 1926; re-printed 1954. Scheler's most important contribution to the subject is *Die Stellung des Menschen im Kosmos*, 1927; reprinted 1947. The concept of 'image' or of 'reality worlds', in a sense more or less related to that of Scheler's investigation, as well as an awareness of the typological variety of 'Weltanschauungen' has of late been widely taken account of in socio-psychological and political thought. Simone de Beauvoir's *The Second Sex* is a brilliant application of the procedure.

times with its *Cogito ergo sum* and *Cogito ergo mundus est.* An appropriate anthropological question here is: Is this a historical process of an increasingly deeper penetration towards the true human condition, or are we rather faced with symptoms of a megalomaniac disease?

Scheler's typology represents five answers to this question. His images of man are those of *homo religiosus, homo sapiens, homo faber, homo dionysiacus,* and *homo creator.* The first of these is based on the Christian-Jewish legacy of super-naturalism with its ensuing feelings of genetic guilt and awe—a legacy in which we all share whether we are believers or not. *Homo sapiens* conceives, in various forms, of the divine plan and of rational man as being in harmony with one another. The fleeting conception of a peculiar historical situation (culminating in the Enlightenment), this view has become dim in the light of another image of man, that of *homo faber,* a rather naturalistic or pragmatic type. Man is now seen as the highest-developed animal, the maker of tools, including language, in whom a particularly high proportion of animal energy is used in cerebral activities. This view overcomes the dualism of body and soul which are now seen as a functional unity. Human being and development are explained by the primarily urges of animal nature, i.e. the libidinous urge for progeny, the urge for power and growth, and the urge to possess and to eat. Indeed, the philosophies of history of Marxism, of racism, of Machiavellanism, and parts of Freudianism are, so it is claimed, based upon this interpretation of man. However, nearly all these naturalistic conceptions still share, with one another as well as with their predecessors, the belief in the unity of human history and the belief in a meaningful evolution in the sense of a development towards higher organisation (Marx, Comte, Spencer, Kant, Hegel, Darwin).

The fourth and fifth conceptions of man break with this tradition. They are both peculiar though radical phenomena of that intellectual situation which heralds a new orientation of anthropological thought. Our fourth picture is that of decadence immanent in human nature and history. With this we move decidedly over to the Continent, to the world of Schopenhauer, Nietzsche, Bergson, of Neo-romantics such as Klages, Spengler and Frobenius. Man, viewed in the light of this anthropology, is the 'deserter of life', the 'faux pas' of life; a megalomaniac species of rapacious apes; an infantile ape with a disorganised system of inner secretion. Compared with other animals, man is essentially deficient in vital powers. He is dependent upon technical means for his survival; and his much vaunted power of thought is an artificial surrogate for his missing or weak instincts. His 'freedom

to choose' is a euphemism for his lack of direction; his social institutions are pitiful crutches to assure the survival of the species which, in a healthy race, would be assured through biological vitality. The state and the law, replacing the instinctual urges of blood and soil, society, instead of community, are symbols of vital impotence. Reason is here regarded as separated from the soul which belongs to the vital sphere of the body; reason is the destructive, demoniac principle struggling with and submerging the healthy powers of the instinctual soul.

Our fifth image of man, that of *homo creator,* brings us even closer to the specific world of PA. This conception, too, is derived from the powerful influence of Friedrich Nietzsche. The idea of the superman has been transformed into a stricter philosophical system by Nicolai Hartmann and Max Scheler. Scheler calls this view a 'postulatory atheism of high responsibility'. We have no ontological knowledge of an ultimate being. However, contrary to Kant's and his predecessors' postulate of the desirability of God, there must and shall now be no God—for the sake of human responsibility and liberty. It is only in a mechanical, non-teleological world that there is room for a free moral being. Where there is a planning all-powerful God there is no freedom for man responsibly to work out his destiny. Nietzsche's 'God is dead' is understood, not in the sense of moral libertinism, but in the presence of an ultimate moral responsibility of man. The predicates of God (pre-destination and providence) are to be related to man. And mark: to man personally and individually, not to humanity, as in the case of Comte, nor to any other collective body. The effective power of the collective structures in history is not deified, as by Carlyle or Treitschke, but they are regarded as the background against which the genuine personality has to measure and prove himself. Those familiar with the existentialism of Sartre, Merleau-Ponty and Simone de Beauvoir will recognise these ideas and will appreciate the response to them outside Germany.

The anthropological significance of the self-image of man consists in the light it sheds on the central concern of PA: to illuminate the whole range of genuine potentialities in man so that his choice of a form of life should not be restricted by narrowness of view.

III

Our next example has to do with the serious consequences which follow from the choice of a form of life too narrow to offer a balanced range of values. Expressed in anthropological terms, we are up against a mode of life modelled on the lines of a partial

anthropology which does not do justice to vital human needs. I am turning to a favourite subject-matter of philosophical anthropologists, the impact of science and industrialisation upon man and society.

The English-speaking world's attitude to technology is in general determined by practical problems caused by technical change. It finds expression in the attempts to bridge the cultural gap caused by alterations in what is an essentially manageable pattern. Some German sociologists of knowledge, on the other hand, hold that the unquestioning belief in the objectively valid world of natural science and technology is a flight into dogma (mechanical and mathematical dogma) of modern man lost in a world without religious allegiance. Indeed, there is a tendency to see demoniac forces confronting us in technology, forces which account for much of the evil we have experienced and which threaten to submerge Western civilisation. In a more playful way, one can discover such ideas in Samuel Butler's *Erewhon,* and one may also think of the views from Burke to Christopher Dawson and Elton Mayo. Many books and symposia have been published in Germany on this subject, the Jünger brothers being the notorious experts. Even so distinguished a sociologist as Alfred Weber has erected into a scholarly theory ideas and anxieties of the type which Aldous Huxley and Orwell made the subject of their literary speculations about the future. However, a philosophical anthropologist such as Gehlen develops a more moderate view, and I shall enumerate some of his more suggestive ideas.[12]

The use of technical means is inevitable for man as he is a vulnerable deficient being. He needs artificial weapons, clothing and tools to survive. And he needs them not only to make up for his deficiencies but also to ease the burden of toil. He also improves on nature by inventing things not seen in the natural world around him, such as certain methods of making fire, the bow and arrow, the wheel, movement by means of explosion, etc. This preoccupation of man with the improving of techniques causes him increasingly to turn towards things, i.e. towards the inorganic world. And, in turn, the occupation with the inorganic

[12] Arnold Gehlen, *Die Seele im technischen Zeitalter,* 1957. This is the most recent statement of Gehlen's position. Gehlen's main work is *Der Mensch, seine Natur und seine Stellung in der Welt,* 1940 (5th ed., 1955). Judging from the impact which he appears to have made on contemporary German philosophical and sociological literature, he may well be the most influential writer in German philosophy and sociology today. See also *Urmensch und Spätkultur,* 1956. Gehlen has certain points of contact with American thought, esp. that of George Mead and of William James (rather than that of such pragmatists as Peirce and Dewey). When Gehlen speaks of the individual and of individual freedom, he thinks in terms of Ihering's and Duguit's form of Organicism This means that the individual is considered to be free only if he merges himself into the wider purpose of a group such as his nation.

leads him progressively into ways of thinking appropriate to the manipulation of the inorganic, into thinking by way of abstract models and mathematical concepts. One result of this has been tremendous (and cumulative) progress on the mechanical side while, at the same time, our understanding of the organic processes of life has hardly advanced beyond the stage of the Greeks. The intellect as we have developed it (one may think of Bergson) comprehends only inorganic acts. Yet the biological and spiritual spheres do not follow the same pattern; they do not respond to the causal-mechanical laws developed by natural science.

How then to arrive at a deeper or at least more comprehensive understanding of technology? It is possible and justified to emphasise the magical element in technology. There is a fascination for man in the automatic processes of nature. He constantly aims to understand his mysterious ego as reflected by the phenomena around him. He identifies himself with animals (Totem) and with shadows. His relationship to the machine is similar. There is a deep urge in man to express himself by means of rhythmic, periodic, automatic processes. Observe a young man on his motor cycle; the tremendous roar, in which he revels, the aimless speed are means of self-expression. The automation of the machine emerges in this view as a value irrespective of the cause-effect relationship. Mechanisation thus comes to be regarded, not so much as freely intended by sovereign rational planning, but as a blind natural process subconsciously developed.

Moreover, the magic impact of automatic processes contributes to the liberating effect of mechanisation. By means of organised, and therefore understandable, activity it liberates man from the paralysing fear (Angst), to which he is helplessly exposed in the face of nature. His instinctive urge to repeat routine actions results in his reducing the world around him to human proportions. The magic force of automation lies in its presenting a copy as it were of the closed structure of the organic human processes such as the regulation of blood pressure, and also of human action. Man applies this procedure of copying the life-processes in creating and controlling inorganic nature. He thus unifies and socialises technology, biology, physics, psychology and sociology,[13] and finally this process finds its climax in the discipline of cybernetics. This leads to a tremendous man-made universe. Yet it regulates only one aspect of human life. These

[13] The limitations inherent in the use of models and, in particular, of analogies, certainly present a genuine epistemological problem. Gehlen's argument, however, also requires to be understood against the background of a rejection (though not a consistent one) of rational behaviour as something devitalised and inferior, in cognitive strength and will power, to animalic, instinctive actions. See *Der Mensch*, 5th ed., 1955, pp. 328 et seq., 353, 381.

man-made copies of life processes do not permit us to come to a deeper understanding. They are of a mechanical nature and present only a functional aspect of a larger whole which greatly transcends this particular field. There is no substantial likeness, there is only structural parallelism.

The one-sided emphasis on mechanical structures has led to essential cultural changes. The instinctive subconscious logic, which has led us to use the seemingly highest conscious processes, has not only affected modes of social and economic life—it has also resulted in a mutation as regards the structure of human consciousness. The purposive or 'zweckrationale' mode of thinking has crowded out and has emancipated man from considerations of value. The experimental character which has come to prevail in thinking is by definition destructive of stable attitudes, obligations, and opinions. Traditional methods, such as family morals, become useless as the logic of the experiment takes over, and the old cultural variety gets replaced by thereotypes.

The structure of experimental thinking does not lead necessarily to a frontal attack on religion and morals. Science and technology are rather 'alienated' from 'Weltanschauungen'. Yet the new style of thinking brings about a substitution of method for substance—method applied irrespective of subject-matter, method for method's sake like *l'art pour l'art.* The aim pursued is not so much a particular social or moral achievement but to find out where the experiment will take us. There is a belief that substantive insights will somehow follow if only we use rational mathematical methods. The new structures in place of the old theories are determined by methods which have become so esoteric that only a small, sometimes a tiny, number of experts understand what is essential for the survival and progress of human society. A new style of thought develops which emphasises terms such as Norm, Standard, Inter-changeability, Momentum, Optimal Economic Effect. Look at politics, morals, and the arts today and you will appreciate the primitivisation which is the counterpart of an esoteric mechanical culture. Unless we consent to being doomed, there is a task for the discarded and outmoded intellectual leaders: this is firmly to demarcate the limits to which mathematical and natural science methods may be used.[14] This task of the cultural personality is, so it appears, not dissimilar from the

[14] However, Gehlen can see occasional merit in the mathematical approach This is so in the case of psychoanalysis which, because it could be universally understood, is said to have exercised a dangerously wide influence, both in its popular appeal and its (unwarranted) impact upon social psychology. *Die Seele,* etc, p. 103. Views similar to Gehlen's in this context are held by other leading sociologists such as L. von Wiese in *Kolner Zeitschrift fur Soziologie,* New Series 3 (1950/51), pp 459-469, and H. Schelsky, *Soziologie der Sexualitat,* 1955, p. 7 and passim.

rôle provided for philosophy by Whitehead in 'Science and the Modern World'.

IV

Our third example will be taken from the strongest group, the psychologists and psychiatrists. Countable by the hundreds, they are to be found in Germany as well as in Switzerland, Holland, and France.[15] They include brilliant names such as Jaspers, Buytendijk, Merleau-Ponty, Binswanger, and their thought is closely related to the philosophies of Dilthey, Husserl, Scheler, Heidegger, and Sartre. What is common to them is the view that traditional experimental psychology requires the assistance of philosophical thought to arrive at satisfactory results. Some of the philosophical anthropologists are radically opposed to the empirical hypotheses and inductive statistical methods of traditional psychology. Most of them, however, combine this traditional approach with their peculiar philosophical or phenomenological method. Their discipline, dealing as it does with individual cases, lends itself more readily to a descriptive approach. While many philosophical anthropologists preferably dwell on generalities, the psychological anthropologists (and the philosophers associated with them) have developed a body of detailed investigations such as analyses of what imagination is, or suffering, laughter, weeping, shame, resentment, pleasure, love, fear, and so on. When I speak of analysis I do not mean the analysis into qualities, elements, and functions based upon sensual perception of real things and events i.e. that analysis which results in a theory and finally in a natural law. It is rather phenomenological 'existential analysis', as Ludwig Binswanger calls his new psychology, which takes its cue from Husserl and from a rather reluctant Heidegger. Binswanger, one of the leading Swiss psychiatrists, has developed his teaching against the background of Freud's psychoanalysis.[16]

Binswanger does not exclude the methods of natural science but he makes two objections to reveal its inherent limitations. One

[15] See S. Strasser, 'Phenomenological Trends in European Psychology', XVIII, *Philosophy and Phenomenological Research* (1957), pp. 18-34.

[16] Binswanger was associated with the rise of psychoanalysis from the beginning, though he never joined Freud's circle. He has recently published a volume of reminiscences of Freud and of correspondence with him. *Erinnerungen an Sigmund Freud,* 1956. In 1936 Binswanger, besides Thomas Mann, was invited to deliver the official address in celebration of Freud's eightieth birthday; he took the opportunity of outlining what separated his teaching from that of Freud. I have, in the text, made particular use of this lecture as well as of other papers contained in Binswanger's *Ausgewählte Vorträge und Aufsätze,* Vol I (1947): *Zur phanomenologischen Anthropologie.* Binswanger's anthropological main work is *Grundformen und Erkenntnis menschlichen Daseins,* 1942. See also Freud's letter to Binswanger quoted in Jones Vol. III, p. 218.

is that all abstractions, even those of 'Gestalt' psychology, are transpositions and simplifications. The other is that they illuminate of necessity only part of the field of investigation, because they are based upon a one-sided philosophical anthropology; i.e. upon an image of man in the world which does not do justice to all the potentialities of man and thus ignores essential elements of diagnosis and cure. To make his meaning clearer, phenomenological perception is not registration of stimuli (as in traditional psychology), but it connotes selective, complementary, and synthesising activities. The perceiving subject constructs a meaningful whole out of the given raw material. We do not perceive sensual perceptions, but we perceive objects towards which we are intentionally directed. By reduction, i.e. by abstracting from subjective and conventional accidental properties, we progress to the intuition of the essential qualities. We perceive meanings which are not accessible to the discursive methods of the natural sciences.

The psychological investigation according to Binswanger's school is directed towards the inner life history of the patient, his self-structuring according to his inner sense or inner motivation. Self-structuring in this sense is equivalent to the response the individual makes to the challenge of the world around him; or to his character which develops in a dialogue with and as an integral part of his inner life history. Take the case of St. Augustine, to whom we owe the beginnings of individual autobiography. Illness prevented him from carrying out his ambition to become an orator. He overcame his disability by turning towards the spiritual world and thus arrived at his essential 'real being'. He could have reacted otherwise: by resentment or frustration, by neurosis, suicide, or rent psychosis (had he been a contemporary of ours). All these and other potentialities offered themselves; or rather the temptation to restrict his character by the impoverishment inherent in an irresponsible choice. But Augustine's answer was the choice of an autonomous life preserving to him access to the full range of human values.

The contrast between the methods comes out clearly in the case of 'heel-phobia'. A five-year-old girl lost the heel of a shoe when it stuck in the skate that was being taken off; she fainted. At the age of twenty-one she developed a phobia, reacting with escape or fainting whenever she sighted a loose heel or heard people talking of heels. Pre-Freudian psychology would have diagnosed that her phobia was caused by the event on the skating rink. Freudian psychoanalysis would rather fasten on the girl's phantasies, the fear of being separated from her mother and of losing a child. The procedure of existential analysis would be as

follows: Everyone suffers the trauma of birth, and many lose heels without getting hysterical. Why then has this particular individual person these phantasies? This is a matter of disposition, which is an anthropological category. It concerns our relationship to the world around us, i.e. our world image, and it concerns the threat which the situation offers to this image. In the case of heel-phobia the decisive category is that of continuity. The patient's world-image is dominated by this category. The event on the ice symbolises the threat of interruption of continuity, the threat of separation from mother and child. The terrifying fear or 'Angst' experienced by the patient is the fear of her world-image being threatened. This process is not conscious or need not be; nor is it subconscious. It is underlying and occasions subconscious reactions —an ultimate framework which may be compared with Kant's transcendental forms of human reason which make experience what it is. The illness is seen as a stereotyped world-image overpowering the patient and making him unfree to choose among the rich potentialities of being-in-the-world. And this leads us back to the very theme of contemporary philosophical anthropology: to establish a complete picture of the potentialities open to man.

Is the massive support commanded by PA amongst Continental psychologists in itself a convincing vindication of its programme? Does the 'anthropological turn' really mark an epoch? Or shall we say that these psychologists share in a provincialism[17] which may also be the hallmark of PA? Has it perhaps, in Hume's words, despite all its suggestiveness, fallen into 'that error, into which many have fallen, of imposing their conjectures and hypotheses on the world for certain principles'?[18]

V

The question naturally arises why British philosophers should acquaint themselves with Continental philosophical pursuits such as PA. Their instinctive reaction is likely to be one of spurning studies which appear not sufficiently critical to be taken seriously by the philosopher, however interesting some of them may be from a religious or anthropological or metaphysical point of view.

[17] So P. R. Hofstatter, *Psychologie*, Fischer Lexikon 6, 1957, p. 9. Regarding Binswanger's existential analysis R. de Rosa speaks of a confused admixture of theoretical, empirical, and philosophical elements. 'Existenzphilosophische Richtungen der Psychopathologie', in *Offener Horizont*, Festschrift fur Karl Jaspers, 1953, p. 189. However, Binswanger's existential analysis seems now to have carried the day among European psychotherapists. See F. E. Heinemann's report on the Fourth International Congress of Psychotherapy in September, 1958, *Philosophy*, XXXIV, 1959, pp. 356-357.

[18] This point has been forcefully argued by J. Kraft, *Von Husserl to Heidegger*, pp 145-146.

However, this situation, far from being novel, falls into a well-established pattern, namely the traditional *bellum internecinum* between the schools which J. S. Mill gave its classical expression in his essays on Bentham and Coleridge. The mutual imputation of intellectual and moral obliquity, of sensualism and mere empiricism, on the one hand, and of mysticism on the other, has been a self-perpetuating quality of this debate. 'They seem to have scarcely a principle or a premise in common. Each of them sees scarcely anything but what the other does not see'. They blink 'the importance, in the present imperfect state of mental and social science, of antagonistic modes of thought . . .'[19]

The large and vast generalities pervading Continental philosophy invite British philosophers to use Occam's razor. Continental philosophers, too, mean to use this tool, though it is apt to be as blunt in the process of phenomenological reduction as it is apt to become the hangman's sword in the hands of many British philosophers. It is, I believe, true that Continental philosophy has often failed in its task to employ an adequate degree of clarity and critical doubt and thus to contribute to a sober general climate of opinion. However, there are sufficient pragmatic reasons for taking notice of the currents from beyond the Channel. I can here only hint at them.

1. Present-day currents determine or, at least, indicate the likely trends of future thought and action, and as ideas, they are apt to penetrate into other systems of thought *some* time. This, for instance, has happened in the cases of Continental sociology and of jurisprudence at a very belated date.[20] If Durkheim, Max Weber, or Simmel are of some importance today in Anglo-American thought, then, no doubt, it will be relevant to watch the development of their thought in their native soil. Unless this is done openly and thoroughly it is certain to be done piecemeal and in disconnected fashion, irrespective of consciously admitted likings and dislikings. It appears to be better to keep abreast of such developments and deal with them on their actual merits and demerits.

2. Metaphysics and ontology are widely regarded as obsolete by British philosophers. But their Continental colleagues discount such statements. Bochenski, for instance, maintains that there are a primitive ontology and dogmatic presuppositions underlying con-

[19] *Dissertations and Discussions*, 3rd ed., pp. 395, 399, and passim.

[20] See Noel Annan, *The Curious Strength of Positivism in English Political Thought*, Hobhouse Lecture No. 28, 1959 Annan's verdict, however, is far too sweeping. A plea for taking into account Max Weber does not entail that his work was of greater importance than, say, that of the Webbs. Another striking example is T. E. Hulme's belated presentation of the thought of Riegl and Worringer.

temporary British philosophy. The same criticism is made by Myrdal, and it has recently, at least in part, been supported by Iris Murdoch and J. O. Wisdom.[21] Continental critics of Hume from Kant to Husserl and to Deleuze have consistently pointed out an alleged metaphysical basis underlying Hume's thought. The excesses of Continental metaphysics and ontology are not excused by these counter-charges. However, if it is true, as I believe it to be, that we cannot escape from making metaphysical assumptions, then there is some merit in facing the issue rather than treating such questions as taboo or accepting without questioning, the presuppositions of scientists. Again, the issue is a traditional one. 'There is no logic to the principles of which we can here appeal. There are different criteria of reality, and what is the reality of one is the mere phenomenon or illusion of the other'.[22] Another counter-charge on the part of Continental philosophers concerns the alleged fact that contemporary British philosophers, too, though paying lip-service to their respect for the results of science, actually take liberties with such results; that, for example, they pay scant attention to relevant psychological insights and put their own 'commonsense' interpretations in the place of scientific theories.

3. The dissimilarities between British and Continental philosophies are obvious. It is, however, possible to observe parallel developments in some respects This is not so surprising in matters of methodology, which in itself is neutral, such as the development of mathematical logic. This approach is more widespread in the English-speaking world, though certainly not neglected on the Continent, where much of it was developed. However, there is probably more scepticism about the limits within which the application of 'heterodox' systems is permissible. Bochenski, for instance, insists on the necessity to use other methods besides the purely formalistic approach, which he wishes to see restricted to operative, abstract systems.[23] Altogether, advanced thinkers within the main stream, such as Bochenski, tend to apply the devices of modern methodology, making use of the investigations of Carnap, Reichenbach Popper, etc. On the other

[21] I. M. Bochenski, *Europaische Philosophie der Gegenwart*, 2nd ed., 1951, p. 83; Gunnar Myrdal, *The Political Element in the Development of Economic Theory*, trsl. 1953, passim; J. O. Wisdom, 'Esotericism', *Philosophy* XXXIV (1959), p 341; Iris Murdoch 'A House of Theory', in *Conviction*, ed Norman Mackenzie, 1958, p. 227. A perceptive treatment of 'the positive picture of the world' is contained in chap. IV of P. Munz, *Problems of Religious Knowledge*, 1959.

[22] J Grote, *Exploratio Philosophica*, II, p 224.

[23] See also C. Lejewski's 'On Lesniewski's Ontology' in *Ratio* I, 1958, pp. 150-151, for an account of Lukasiewicz's and Lesniewski's attitude towards 'mathematical games' and 'pure formalism'.

hand, a large number of philosophers believe their appropriate preoccupation to be something basically opposed to science; they accuse each other of 'atomistic' or 'mechanical' or 'abstract' thinking as often and as mutually as, for instance, British philosophers have, in another context, applied the device of the 'naturalistic fallacy'. Yet again, more thoughtful philosophers such as Dilthey, Jaspers, and Binswanger favour a complementary use of the methods of natural science and of phenomenology or similar methods.

Furthermore, it is worth pondering how far the 'spirit of the age' finds expression irrespective of different lines of approach. It it intriguing to compare the work of a leading philosophical anthropologist such as the Swiss zoologist Portmann[24] with that of Gilbert Ryle. Their working canons are distinctly different. Yet their aims are closely related to each other. Both reject the methodological division between the natural and the social sciences. Both aim at overcoming the Cartesian separation of body and mind. Both wish to expose the misleading 'anthropologies' underlying the approaches of the different sciences such as biology, psychology, sociology, ethnology, etc. Both therefore develop some sort of psychology of their own, Portmann in the hermeneutic tradition of 'geisteswissenschaftliche' psychology of Bergson and Dilthey, Ryle by means of linguistic analysis. For Portmann the subject-matter of thought is man as a mysterious unit. For Ryle man is not to be explained in a metaphysical or mechanical fashion; yet, 'there has yet to be ventured the hazardous leap to the hypothesis that perhaps he is a man'.[25]

There is another direction in which similarities in trends may be observed. The climate of opinion in British political philosophy has for some time been anti-theoretical both in liberals such as Weldon and in conservatives such as Oakeshott. This has led to a relativist and conformist acquiescence in the 'status quo'. The same attitude, in the wake of the romantic Historical Schools, has been widespread on the Continent where lawyers, historians, and philosophical anthropologists (though not Scheler amongst those treated here) have excelled in supporting the 'status quo'. However, on the British side the case of the 'good life' has gone by default—ostensibly, as not presenting an appropriate subject-matter for philosophy and, practically, because most people have come to accept the rational solutions of the utilitarian tradition.

[24] A. Portmann, 'Um eine basale Anthropologie', *Wirtschaft und Kultursystem*, 1955, pp 286-300; *Zoologie und das neue Bild des Menschen*, 1956, with further bibliography pp. 137-138.

[25] *The Concept of Mind*, p. 328.

Continental philosophers, on the other hand, have felt tempted actively to glorify irrational choices.

4. Though there are similarities they are, however, inadequate for bridging the gap between the respective traditions. If I may try to point to what may be the essential difference between these traditions, it appears to me that modern British philosophers have predominantly concentrated on the refinement of methods and rules while Continentals have attempted to wrestle with substantive assumptions. British philosophers are therefore appalled by the poverty of critical acumen in some representative Continental thought. And Continental philosophers are puzzled by what appears to them the lack of commitment to vital ideas and the triviality of subject-matter in British philosophy. Both sides feel that criticism from the other end does not strike at the relevant elements of their thought. At the same time, it becomes clear where they may complement each other. Continental philosophers are in need of British methodological clarity and exactness. But British philosophers can do with Continental eagerness in asking questions and with some scepticism regarding the border-line beyond which clarity is bought at the price of the exclusion of meaningful and relevant questions. As a matter of fact, there is a discernible trend in recent British philosophy towards a renewed stronger interest in the 'great issues' of the human condition, away from the fascination with the technicalities of analysis or with the material world.[26]

It is comparatively easy to arrive at agreement regarding techniques and rules. It is more difficult to reach understanding on what is substantially relevant and 'important'. It is in this respect that PA has something of interest to offer, namely its variety of imaginative and possibly fertile visions and interpretations. However, its adherents fail as a result of a frequently insufficient submission of their theories to severe criticism and to severe tests. This weakness provides good reason for rejecting much of their thought and for a sceptical scrutiny of all their propositions in the context of their problem-situations. Yet this is no reason to by-pass *in toto* their tentative contributions to such questions as the autonomy of ethics, the philosophy of history and of religion, and the relationship of philosophy and myth. There is,

[26] See J. A. Passmore's review of Warnock's *English Philosophy since 1900* in this *Journal* 37, 1959, pp. 79-81. P. F. Strawson's *Individuals* and Stuart Hampshire's *Thought and Action* are cases in point. Popper's philosophy may serve as an example of a judicious fusion of what is best in the two traditions. The same view could be expressed regarding J. S. Mill's philosophy. The influence of earlier British thought on Continental philosophy and sociology is generally underestimated, largely because of the lack of sympathy shown by many contemporary British philosophers for some significant aspects in the thought of their predecessors.

however, one important point to make in this context. It is not quite fair to contrast Continental philosophy with British philosophy as such. As Continental philosophers cast their nets more widely a comprehensive comparison must take note of Anglo-American anthropology, psychology, and generally of the behavioural sciences. If this is done it will be found that most of what Continental philosophy may have to offer has been absorbed and, indeed, put to more critical use. It may be said that British philosophy has erred by excluding, and Continental philosophy by usurping the subject-matter of the sciences. If this points a moral it is that the very idea of specialisation requires to come under review. Though English-speaking philosophers have been impervious to Continental irrationality they may have been too credulous in emulating the image of the traditional German method- and system-ridden scientist, the successor of Browning's grammarian.

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ARE METAETHICAL THEORIES NORMATIVELY NEUTRAL?

By WILLIAM T. BLACKSTONE

A. J. Ayer has argued that "all moral theories, intuitionist, naturalistic, objectivist, emotive and the rest, in so far as they are philosophical theories, are neutral as regards actual conduct".[1] These analyses of moral concepts or metaethical theories do not dictate or recommend certain kinds of conduct. They entail no normative ethical judgments. Metaethical theories are only analyses of "what people are doing when they make moral judgments".

Since the time of Ayer's statement, a number of philosophers[2] have become concerned with the relation between metaethics and the moral life or the relation between analyses of the meaning of moral concepts and normative ethical judgments. They have asked the question: "Are metaethical theories normatively neutral?" This question has been answered differently by different philosophers. In fact, the question itself has been interpreted as asking different things. The purpose of this paper is to set forth six fundamentally different interpretations of the question, "Are metaethical theories normatively neutral?". We will also propose answers to each of these formulations of the question. It is hoped that this procedure will clarify the relationship between metaethics and normative ethics.

I

The question, "Are metaethical theories normatively neutral?", has been interpreted as "Do metaethical theories affect one's moral life?". Olafson,[3] for example, argues that the acceptance of a metaethical theory does affect our first-order moral life and that this effect is observable in a modification of our procedures of moral judgment. He is primarily concerned with the emotive theory and it is his specific contention that the acceptance of the emotive theory changes our conception of what we are doing when we make moral judgments, and further that it changes our view of the justification of such judgments. The emotive

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[1] A. J. Ayer, *Philosophical Essays*, p. 246.

[2] For example, see Frederick Olafson, "Metaethics and the Moral Life", *Philosophical Review*, vol. 65, 1956; Paul Taylor, "The Normative Function of Metaethics", *Philosophical Review*, vol 67, no. 1, January, 1958; and S. A. Grave, "Are the Analyses of Moral Concepts Morally Neutral?", *The Journal of Philosophy*, vol. 55, no. 11, May 22, 1958

[3] Frederick Olafson, *op cit.*

theory, as a metaethical theory, forces us to recognize the primacy of emotion in morals. This "performatory analysis of moral judgments generates a performatory moral life".[4] Acceptance of the metaethic of emotivism causes psychological or attitudinal changes in one. It makes one feel that rational principles are not operative for morals and that one's moral judgments are non-logical acts of preference.

Olafson, I think, is correct in his contention that acceptance of the emotive theory causally affects one's attitudes in morals. Although the emotive theory need not entail nihilism or chaos in ethics,[5] there is good evidence that acceptance of the view that moral judgments are mere expressions of emotion causes one to adopt a different attitude toward justification in ethics, and may in fact cause one to make different normative ethical judgments from those made prior to the acceptance of the metaethic.[6] It seems to me that the acceptance of any theory involving meta-linguistic analysis results in such causal effects. Certainly the meta-linguistic statement that religious or theological statements are non-cognitive has a causal effect upon one who operates within a religious framework. The belief that religious utterances can be neither true nor false has very serious causal effects when entertained by one who has religious beliefs, for such a meta-linguistic statement is even more devastating than being told that one's religious beliefs are false. It surely makes one feel as if no rational principles are operative in religion and that one's religious judgments are non-logical acts of preference.

Our point is that philosophical analysis of any area of discourse has a number of practical causal effects and this certainly includes analyses of moral concepts. It is certainly possible and in fact highly probable (though not logically necessary) that acceptance of a given metaethic, whether it be the emotive theory, the subjectivist theory, or the view of the intuitionist, will have a tendency to modify one's behaviour. It may (though it need not) result in one's making different normative ethical judgments from those made prior to the acceptance of the metaethic. Thus our answer to the first formulation of our question, namely, "Do metaethical theories affect one's moral life?", is a probable Yes.

II

The question, "Are metaethical theories normatively

[4] *Ibid.*, p. 175.

[5] See W. T. Blackstone, "Objective Emotivism", *The Journal of Philosophy*, vol. 55, no. 24, November 20, 1958.

[6] See Paul Edwards, *The Logic of Moral Discourse*, p. 240, for an analysis of the consequences of adopting the emotivist metaethic.

neutral?", can also be put in this manner: "Do one's normative ethical beliefs logically entail one's metaethical theory?".

A look at the history of ethical theory will convince one that the normative ethical views of some philosophers certainly did dictate their metaethical theories. Let's take one such example. Bentham makes the following statement: "Of an action that is conformable to the principle of utility, one may always say either that it is one that ought to be done, or at least that it is not one that ought not to be done. . . . When thus interpreted the words *ought* and *right* and *wrong,* and others of that stamp, have a meaning: When otherwise, they have none".

Bentham's commitment to the principle of utility has led him here to assert the metaethical thesis that the normative terms "ought", "right", etc., could mean nothing other than that specified by the utilitarian. His thesis here is one about the meaning of ethical terms, not a thesis about what we ought to do. But it is clear that his position in normative ethics—that we should act so as to increase the happiness and welfare of all people—is the foundation or source for his metaethical thesis. Bentham's metaethical analysis of the meaning of "right" or "ought" is a reflection of his own ethical evaluations, and his preclusion of other metaethical theories is a logical consequence of his acceptance of the normative ethical views of utilitarianism. He has so defined the "ethical" that an act *must* conform to the principle of utility to be an ethical act. This makes his metaethics a logical entailment of his normative ethics.

It is no doubt true that the normative ethics of many other historical ethical theorists have dictated their metaethical views. However, a metaethical theory need not be a reflection of one's ethical evaluations. One need not arbitrarily define ethical concepts in terms of one's own normative ethical commitments such that one's normative ethics logically entail one's metaethics. That is, one's metaethics can be morally neutral even though it is a historical fact that many metaethical positions are not. The "ethical" or terms like "ought", "right", etc., could be defined in terms of use and function—in a morally neutral manner. One could view "ethics" as any system of norms for human conduct—a system of rules which function as regulative devices. Viewed in this manner, the subject matter of metaethical analysis would include the codes of behaviour advocated by anyone—those of Nietzsche, Kant, Mill, St. Paul, Hitler, etc. A metaethic of this type would be a theory about the meaning of all distinctly ethical terms and statements (any term or statement which functions in this regulative sense) and this metaethic would not be a logical entailment of any given normative ethics or a reflection of one's

own ethical evaluations. A metaethic of this morally neutral type would be a result of an analysis of the features and functions of discourse in which terms and statements function in a normative, regulative sense concerning human conduct. A metaethicist may, for example, conclude from his analysis that all terms and statements which function in a normative, regulative sense are non-cognitive expressions of emotion (the metaethic of emotivism). This conclusion would apply equally to the normative ethical principles of Nietzsche, Kant, Mill, St. Paul, Hitler, etc., and would not be a logical entailment of any given normative ethics. Our answer, then, to the second formulation of our question, namely, "Do one's normative ethical beliefs logically entail one's metaethical theory?", is that although this has been true of many historical ethicists, one's metaethics can be morally neutral.

III

The question, "Are metaethical theories normatively neutral?", can be and has been interpreted in this manner: "Do metaethical theories entail certain normative ethical statements or moral claims?". For instance, is it the case that the metaethical view that moral judgments are non-cognitive expressions of emotion entails any particular normative ethical judgments? Or does the metaethic of subjectivism, the view that moral judgments are statements of individual approval or disapproval, entail any particular normative ethical judgments? Ayer's answer to this question is negative and I agree with him. Any given metaethic, including the emotive theory, aims at showing people what they are doing when they make moral judgments—not at suggesting which moral judgments they are to make. Furthermore, without logical inconsistency, one can be for or against the same sort of actions regardless of whether one adopts the metaethic of the emotivist, the subjectivist, objectivist, or intuitionist. One is not logically committed to capital punishment, trial marriage, or euthanasia—no matter what metaethic one adheres to. This is to say that there is no formal incompatibility between any normative ethical statement and any metaethical theory. This conclusion, however, requires that one's metaethical theory be morally neutral in the manner specified above, namely, that it not be a reflection of one's own normative ethical position. It should be noted, however, that the acceptance of any given metaethic may *cause* or influence one to accept a certain moral position. Our answer, then, to the third formulation of our question, "Do metaethical theories logically entail certain normative ethical statements?", is negative.

IV

A fourth formulation of the question, "Are metaethical theories normatively neutral?", is "Do metaethical theories logically entail certain accounts of moral justification?". Metaethical theories are theories about the meaning of ethical terms and ethical statements, and, it seems to me, any given theory about the meaning of ethical terms and statements does logically entail a particular account of moral justification. The metaethic of subjectivism, for example, which views the meaning of ethical statements as autobiographical statements of approval or disapproval, entails a particular account of moral justification, namely, that the only data relevant to the justification of moral judgments are the autobiographical facts of personal approval or disapproval. The metaethic of emotivism, which views ethical judgments as non-cognitive expressions of emotion, certainly logically entails that moral judgments cannot be justified as true or false since moral judgments do not *assert* anything. For those who adopt the metaethic of emotivism, if there is disagreement in moral attitude between two persons who agree on the facts of the case, there is no method of resolvement besides persuasion or abuse. On the emotivist's scheme, all the reasons relevant for a *purely* moral disagreement (as opposed to a disagreement in belief) are persuasive reasons, for, as Ayer puts it, moral principles have no "objective validity". Thus one analytic entailment of the emotive metaethic is that all ethically relevant reasons are persuasive reasons. Without noting other metaethical theories, it is clear that the manner in which one is to justify moral judgments depends logically upon what those ethical judgments are interpreted as meaning. Our answer, then, to the fourth formulation of our question is affirmative.

V

A fifth sense of the question, 'Are metaethical theories normatively neutral?", is "Are metaethical theories set forth as descriptively true theories or as prescriptions concerning the way moral language ought to be used and interpreted?". (We have already discussed above how a metaethic may be prescriptively set forth in the sense of being a logical entailment of a given normative ethic. We are here concerned with a different manner in which a metaethic may be prescriptive.)

If metaethical theories are set forth as descriptively true theories, the question arises as to what it is that they describe. Quite often a metaethicist maintains that his theory is a descriptively accurate account of the way that moral language is used.

The emotivist, for example, often seems to make this appeal to the way that moral language is used. If the emotivist's metaethic is based on this appeal, then it seems that it is descriptively false, for moral discourse certainly employs all the devices characteristic of cognitive discourse. As Glassen[7] points out, moral judgments often take the form of indicative sentences just as do other sentences known to be cognitive. Moral judgments also often appear in indirect discourse as the object of a cognitional verb like "know" or "believe". One quite often finds judgments like "I know that x is the right thing to do". Furthermore, appraisal terms like "true", "false", "correct", and "mistaken", are often applied to moral judgments. Thus we find persons saying: "It is true that x is right". This linguistic evidence *prima facie* supports a cognitive interpretation of moral discourse, and hence the appeal to "use" does not support the emotivist's metaethic.

However, the emotivist also maintains that his analysis is the correct analysis of what moral concepts *really* mean and what these concepts really mean is quite different from what they are taken as meaning in ordinary moral discourse. Ethical expressions are *really* expressions of emotion, not statements which can be true or false. But what are we to make of the emotivist's contention that his view is the correct interpretation of the *real* meaning of ethical concepts? What he appears to be doing is setting forth his metaethic as a prescription of how we should interpret and use moral language. This means that his metaethic is not normatively neutral in one sense of "normative neutrality". His metaethic, requiring that we view moral language as emotive, is based on a norm which states what should be accepted as being cognitively meaningful. The phrase, "real meaning of ethical terms", involves a reference to this norm. In the case of at least some emotivists, that norm is the principle of empirical verifiability. The metaethic of intuitionism rests upon a different criterion of meaning from that of the emotivist, and hence the prescription of the intuitionist concerning the way that moral language ought to be used and interpreted differs from that of the emotivist. In neither the case of the intuitionist's metaethic nor the case of the emotivist's metaethic is an appeal made merely to a descriptive account of the way that moral language is used. Our answer, then, to the fifth formulation of our question, "Are metaethical theories normatively neutral?", is negative.

VI

A sixth possible interpretation of the question, "Are meta-

[7] See Peter Glassen, "The Cognitivity of Moral Judgments", *Mind*, vol. 67, no. 269, January, 1959; and Frederick Olafson, *op. cit*

ethical theories normatively neutral?", is "Does metaethical analysis have a normative function?". Paul Taylor[8] has argued for an affirmative answer to this question and I think that he is correct. The normative function of metaethics is to introduce greater rationality into our moral life. This function is fulfilled if it makes "our second-order beliefs about our first-order moral discourse more clear, coherent and true. If a metaethical analysis can correct our second-order errors and clear up our second-order vagueness and confusion about the nature and logic of our moral experience, then it can in turn help us to be more rational in our first-order moral deliberations and judgments".[9] For Taylor the criterion for a valid metaethic is whether that theory has the capacity for making us more rational in our moral life. A metaethic has that capacity if it satisfies the following tests: (1) "Does it increase our factual understanding of what happens when, in the ongoing practical situations of everyday life, we deliberate about moral issues, arrive at moral decisions, make and justify and argue about moral judgments, and in general carry on moral discourse with others?" (2) "Does it provide tools for analyzing moral discourse in such a way that we are better able to recognize the presence of ambiguity and vagueness in moral language, and to identify the ways in which, and the purposes for which, such language is used?" (3) "Does it enable us to carry on first-order moral discourse more clearly and intelligently?" (4) "Does it make explicit the rules of valid reasoning which are appropriate to moral argument; and does it make clear in what respects these rules are similar to, and in what respects they differ from, the rules of valid reasoning appropriate to matters of fact and to logically necessary propositions?" (5) "And finally, does it show us clearly how these rules can be applied in the everyday situations in which moral problems arise?"[10]

It seems to me that tests (2), (3), (4), and (5) are all heavily dependent upon one's answer to (1). If we are to be better able to recognize the presence of ambiguity and vagueness in moral language (2), we must have a clear understanding of what really goes on in moral discourse. Carrying on first-order moral discourse more clearly and intelligently (3) also requires an answer to (1), namely, a view of what really goes on in moral discourse. Furthermore, knowing what could be meant by "rules of valid reasoning appropriate to moral argument" (3) presupposes an answer to (1). If what really occurs in moral discourse is the expressing of emotion, then the meaning of "valid

[8] Paul Taylor, *op. cit.*
[9] *Ibid.*, p. 28.
[10] *Ibid.*, p. 29.

reasoning" in ethics is radically affected. Finally the application of rules of valid reasoning to moral problems (5) requires an answer to (4) which in turn requires an answer to (1). Taylor's proposed test for the acceptability of a metaethical theory, then, seems to boil down to whether the theory "increases our factual understanding of what happens when, in the ongoing practical situations of everyday life, we deliberate about moral issues, arrive at moral decisions, make and justify and argue about moral judgments, and in general carry on moral discourse with others".

It appears to me to be analytically true that if a metaethical analysis provides this "factual understanding", then it fulfils the normative function of providing the *capacity* for making us more rational in our moral life (though not necessarily actually making us more rational). But is it not the case that all metaethical analyses are set forth as "factual understandings" of what really happens in moral discourse? How do we know which metaethic fulfils this normative function? In other words, how do we determine what constitutes a "factual understanding" of moral discourse? This question can also be formulated in this manner: What is the test for the "real meaning" of ethical terms and ethical statements?

Would a descriptive account of the way moral language is used tell us what really happens in moral discourse or give us the *real* meaning of ethical terms? Taylor answers this question affirmatively. Speaking of moral concepts, he states: "A metaethic simply tries to get us to be fully aware of what this use is and to know what to do when others challenge our use of moral expressions or when we become doubtful about our own use of them".[11] In becoming aware of the use of moral concepts, one becomes aware of their meaning. A valid metaethic, then, is apparently for Taylor one that correctly describes the way that moral concepts are used for the rôles that they perform in language.

Taylor's implicit criterion for judging whether a given metaethic constitutes a "factual understanding" of moral discourse seems to be based upon the implicit acceptance of the identification of meaning with use. We can discover what moral concepts really mean by examining their use or the rôles that they perform in language. If this criterion is employed, then it would seem that each of the metaethical theories—subjectivism, objectivism, emotivism, and intuitionism—is partially correct. Moral concepts and moral language have many functions or rôles. Moral language often performs the function of expressing one's emotion (the function stressed by emotivism). It quite often performs the

[11] *Ibid*, p 31

function of conveying autobiographical information (the function stressed by subjectivism). It also is quite often used to make assertions which are not autobiographical (the function stressed by the various forms of objectivism). Thus if we accept the Wittgensteinian identification of meaning with the use or rôles that expressions play in language, it may well be that intuitionism, objectivism, subjectivism, and emotivism are each correct in describing one rôle or function of moral language but each is incorrect in viewing that rôle as the exclusive rôle or function of moral language. On this procedure, a correct metaethic would be one that displays all the uses or rôles performed by moral concepts. A metaethic of this type will be more likely to provide clear, coherent, and true second-order beliefs about our first-order moral discourse, and help us to be more rational in our first-order moral deliberations.

The point I wish to make is that any answer to the question of what constitutes a "factual understanding" of moral discourse is logically related to the general theory of meaning that one accepts. Without arguing the point, it seems to me that the metaethic of emotivism is often logically related to the acceptance of the positivist's theory of meaning. It is also clear that the implicit account of meaning in Platonism produces a quite different metaethic from that which results from the acceptance of the positivist's theory of meaning, that which results from the position which identifies meaning with denotation, or that which results from the acceptance of the Wittgensteinian account of meaning. It seems clear, then, that an answer to the question of whether a given metaethic fulfils the normative function of providing us with the capacity for becoming more rational in our moral life (by providing us with a "factual understanding" of moral discourse) ultimately requires reference to a general theory of meaning. Since this is the case, then it seems to me that metaethicists should pay a great deal more attention to the problem of meaning than they have in the past and to the particular problem of criteria of adequacy for a valid theory of meaning.

In summary, we have examined six different interpretations of the question, "Are metaethical theories normatively neutral?". These interpretations of this question were: (1) Do metaethical analyses affect one's moral life? (2) Are metaethical theories logical entailments of normative ethical positions? (3) Do metaethical theories logically entail certain normative ethical statements? (4) Do metaethical theories logically entail certain accounts of moral justification? (5) Are metaethical theories set forth simply and solely as descriptively true theories rather than as prescriptions concerning the way moral language ought to be

used and interpreted? (6) Do metaethical theories have a normative function? Our answers to (1), (4), and (6) were affirmative, while our answers to (3) and (5) were negative. Our answer to (2) was affirmative for some historical cases, but we insisted that a metaethical theory need not be a logical entailment of a normative ethical position. Our discussion of (5) and (6), however, indicated that the acceptability or non-acceptability of a metaethical theory is closely related to the acceptability or non-acceptability of a general theory of meaning. The problem, then, of justifying a general theory of meaning is a paramount task for any metaethicist, for to speak of a "valid metaethic" requires that one speak of a valid theory of meaning.

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ON THE ARGUMENT OF THE PARADIGM CASE*

By ROBERT J. RICHMAN

Much has been said of late about the argument of the Paradigm Case,[1] which I shall refer to for brevity as the APC—although I do not think highly of it as a philosophical analgesic. That this argument has been so widely discussed is due in large measure, I suspect, to the fact that "Oxford" philosophers rarely make explicit the general principles on the basis of which they carry out their (frequently brilliant) philosophical inquiries, with the result that when one of their number does attempt to set forth one of these principles, his effort is likely to arouse a great deal of attention. Trusting that adverse criticism will not discourage further attempts at explication of principles, I shall argue (a) that the APC is not a conclusive argument and (b) that it is of little *philosophical* importance.

I

Unfortunately, Urmson does not explain the APC with complete clarity. He does, however, explain its function:

"By it the philosophical doubt whether something is really an X is exposed as being in some way improper or absurd by means of a demonstration that the thing in question is a standard case by reference to which the expression 'X' has to be understood, or a doubt whether anything is X is exposed by showing that certain things are standard cases of what the term in question is designed to describe."[2]

He also sets forth examples designed to clarify the nature of the APC.

"Suppose that someone looking at what we would regard normally as a typically red object expressed a doubt whether it was really red. He might indeed express doubts whether it was really red because he thought that the light was unusual, or that his eyes were bad, or something of that sort. But suppose that he expresses doubt for none of these reasons but doubts whether the term 'red' can properly apply to this sort of thing. We would then be at a loss and probably ask him what on earth he meant by red if he was unwilling to call this red, or say that by 'red' we

* Read at the Christmas meeting of the Pacific Division of the American Philosophical Association at the University of Oregon, 1958.

[1] Set forth in J. O. Urmson's "Some Questions Concerning Validity", reprinted in *Essays in Conceptual Analysis* (London, 1954). For discussion, see, e.g., *Analysis*, Vol 18, Nos. 2, 4, 5, and 6.

[2] *Ibid*, p. 120.

meant being of just some such colour as this—'If we do not call *this* red then what would we?' Thus using a simple form of the argument from standard examples, we can make him see that there is something absurd in his question, since there is no better way of showing what the word 'red' means than by pointing to things of this colour."[3]

I have quoted this trivial example (Urmson's description) at some length since I am trying to become clear about the nature of the APC, and because the last sentence of the quotation seems particularly useful in this attempt. The APC appears to be closely related to the doctrine of ostensive definition. Its nature may be indicated as follows: The meaning of some term 'X' is learned by being shown things t_1, t_2, etc., which have the property X. These things, t_1, t_2, etc., are then standard examples, or paradigm cases of X. Since this is so, it is absurd to question whether, say, t_1 is an X, or whether anything is an X.

First, a word about the term 'absurd'. If the APC is to have more than merely persuasive value, the 'absurd' of its conclusion must be a logical or quasi-logical predicate (meaning something like 'self-contradictory'). 'Absurd', in other words, must not be merely a pejorative term indicative of our dislike or disbelief of the conclusion; otherwise, the APC is not an *argument*. We cannot dispose of opposing views by calling them foolish.

Why, then, might it not be absurd to assert with regard to some predicate 'X' such as is referred to in the APC that nothing (really) is X, or that some particular thing is not X? Why, in other words, is the APC less than conclusive? Clearly, if we learn the meaning of 'X' by being shown things which are X, and if we have learned what 'X' means, then there are things which are X. This follows from the meaning of 'show': we can't *show* someone things which are X unless there *are* things which are X.

Phrasing the argument in terms of what words *mean* may suggest that the APC is a completely semantical or logical sort of argument. But this suggestion is quite misleading. The premises of the argument are not logical in character—rather, they are psychological. They have to do with the way in which the meanings of certain terms are said to be shown or learned, and there is no logical absurdity involved in supposing that they are false. Most radically, there is no logical absurdity involved in supposing that we don't *learn* the meanings of any terms at all. It is conceivable that we might at birth, or through a process of maturation, understand the meanings of terms generally, or of some particular terms. On this supposition, it is obvious that

[3] *Ibid.*, p. 121.

some or all terms which we understood might have no *denotata*. But this supposition, while not perhaps logically absurd, is, I suppose, believed nowadays by no one. I shall therefore disregard it, except to point out that it alone is sufficient to show that the conclusion of the APC is not proved by the argument.

Let us assume that we do learn the meaning of terms. This assumption is obviously insufficient to sustain the APC, since the learning process might in every case be unlike that indicated in the APC. I wish to emphasize the contingent character of the supposition that we learn the meaning of (some) terms by being shown instances to which it applies. (I am not asserting that this suggestion is false. Neither, incidentally, am I denying that some things are red, that my desk is solid, or that John Foster Dulles exercises free will. But I do not think that any of these assertions can be conclusively demonstrated by the APC: anyone who seriously doubts any of them will be unconvinced by the APC, and not illogically so.) There is involved a question of psychological causation, not to be resolved *a priori*. It *might* be the case that we are led to understand the meaning of 'red' by being shown round objects, green objects, or elephants, by intravenous injection, or by surgical operation. As a matter of fact, I don't suppose that our knowledge of physiological psychology is sufficient to rule out the last two methods as physically impossible. But let us admit that we know what 'red' means, and that we haven't in fact learned this in any of these ways. Recognizing that the case of a term like 'red' (i.e., any term whose meaning can presumably be shown *only* ostensively) is a kind of paradigm case of the applicability of the APC, what could a person say against the APC if he wanted to deny, say, that anything is red? (We shall assume that our straw man makes use of none of the possibilities which we have already rejected, e.g., that the meaning of 'red' is not learned, or that it is learned in some logically possible, but odd way.) We can get a clue as to what he could say from the manner in which he would presumably formulate the statement of his position: "Nothing is *really* red". Granted a doctrine of ostensive definition, granted that the meaning of some terms is taught by exhibiting examples, still all that is required to show the meaning of 'red' are cases of *apparent* redness. As long as we can learn the meaning of 'red' from things that appear red, it cannot be logically absurd to assert that we have learned the meaning of 'red' ostensively and to deny that red things exist. The sceptic's position here is rendered strong by the semantical fact (a prime stumbling-block, incidentally, to phenomenalism) that for no characteristic ϕ is it the case that 'x appears to be ϕ' is synonymous with or entails 'x is ϕ'.

One more word about 'red' (and similar terms). Suppose that our hypothetical sceptic were to deny not that anything is red, but that something, t, which *we* took as a paradigm case of redness, is red—and that he expressed no doubts about the lighting conditions, his eyesight, or the like. The situation would clearly be a difficult one. For if t represents a paradigm case of redness then *a fortiori* it represents a case of redness, i.e., t is red. But conversely, if someone denies that t is an example of a red thing then, of course, he denies that it is a *standard* example or a paradigm case of a red thing. Thus, Urmson's assertion that we can make the sceptic see the absurdity of his position by applying the APC seems unduly sanguine. There is needed some support for the contention that t *is* a paradigm case. There is needed *at least* a way of telling in controverted cases that an example we are dealing with is a standard example. What is the criterion which we employ for determining that a given putative case of 'X' is a *paradigm* case? This is an aspect of the APC which surely requires clarification. If, as the quotation from Urmson on 'red' suggests, a paradigm case is literally one on the basis of which the meaning of a term can be shown, then on the basis of *this* conception a paradigm case of 'red' might not be an example of a red thing at all. To see this, consider what happens in showing or teaching the meaning of 'red'. The child—as far as I know the ostensive teaching of the meaning of 'red' occurs almost invariably in childhood—comes to understand this term in part by being shown red objects. So far, so good. But because of lack of precision, and also because of inattention on the part of the teacher, many of the things on the basis of which an understanding of the meaning of 'red' is acquired may not be red. (I ignore, of course, the fact that part of coming to understand 'red' consists in being told that certain objects are not red; in a sense these objects are part of the basis on which the meaning of 'red' is shown.) The first point is of greater interest for our present purposes. In teaching the meaning of a term we may make use of examples which may no longer serve as examples as the concept becomes more precise. This point is clearer in other cases, but even in the case of 'red' the meaning of the term may be shown in part by pointing to things of which we should say, after our concept of red has become rather more refined, "No, that's not quite red—it's rather a shade of pink". Thus, e.g., t might be a paradigm case of redness (in the sense in question) and not be red, but rather, say, pink. Presumably then *this* is not the criterion of a paradigm case intended (but what does Urmson mean by the curious phrase "showing what the word . . . means"?), and the question as to how to recognize a paradigm case remains open.

So far I have dealt with the application of the APC to terms like 'red' which presumably are definable only ostensively. These are the cases in which the cogency of the APC is greatest, and yet even here there are grounds for denying that the APC is conclusive Bearing in mind that the objections raised apply *a fortiori* to other sorts of cases, let us drop this line of inquiry since, APC or no APC, there is little if any disagreement about the applicability of terms like 'red'—disagreement, that is, of the sort which the APC is designed to resolve.

Let us look briefly at another example of the use of APC which Urmson cites. This example has the advantage of being directed against someone, namely, the physicist Sir Arthur Eddington. Urmson writes:

"Eddington said in effect that desks were not really solid. Miss Stebbing, in her book *Philosophy and the Physicists,* used the argument from standard examples to show that this way of putting things involved illegitimate mystification; this she did by simply pointing out that if one asked what we ordinarily mean by *solid* we immediately realize that we mean something like 'of the consistency of such things as desks' "[4]

This last statement clearly won't do. What we ordinarily mean by 'solid' has no particular reference to desks;[5] we would be able to use the word quite intelligently if no furniture at all existed. Whatever the difficulties with the concept of analyticity, it is reasonably clear that 'this desk is solid' is not analytic. (One might be tempted to say that this is a paradigm case of a synthetic statement!) Yet, if 'solid' is synonymous with 'of the consistency of such things as desks', then 'this desk is solid' would be analytic.[6] (Note that different applications of the APC may lead to contradictory results.)

But it may be urged (a) that to say that this desk (e.g., Eddington's) is not solid is misleading and (b) that if we don't call this desk solid then "what would we?". As to (a) saying that the desk is not solid may be misleading because of an ambiguity in the term 'solid'. Among other things, solid' means (roughly) (1) 'without spaces or interstices' and (2) 'substantial, not flimsy'. It is clear from the context of Eddington's book that he is concerned with solidity in the former sense. If the assertion that the desk is not solid suggests, e.g., that the desk won't support such things as lamps and books, it is only because 'solid' is being taken in the second sense. Thus, Eddington's statement, read in context, *ought*

[4] *Ibid.*

[5] Indeed, Eddington writes not of desks but of tables

[6] I assume that the use of 'this' obviates difficulties connected with considerations of existence. At any rate, 'all desks are solid' could serve as our example.

not to be misleading. (The suitability of a "realistic" interpretation of micro-physics is another question.) Moreover, as perhaps this example suggests, a statement can be misleading without being false (there are, of course, moral problems connected with *deliberately* misleading assertions), and almost any statement can be misleading to *some* individuals. As to (b) it might well turn out that there exists nothing which Eddington would call solid. This would show not that 'solid' has no meaning, but only that it has no denotation. If 'solid' means 'without interstices' it might happen that nothing satisfies this description; but we *would* call solid anything which did. (There is a question of the *usefulness* of this way of talking; I should suppose that this is a separate question from that of its absurdity.)

II

Urmson's main point with regard to the APC in "Some Questions Concerning Validity" is not to defend it but to indicate a restriction on its applicability. I should like to argue quite briefly that if this restriction is necessary then (a) this is further evidence of lack of cogency of the APC and (b) the APC is rendered of little philosophical importance. The restriction on the use of the APC has to do with value terms: according to Urmson, the APC cannot legitimately be used to demonstrate the applicability of such terms since, in effect, to go from the factual premise that a value term is applied to certain things to the conclusion that the things in question are valuable is to be guilty of the Naturalistic Fallacy.[7]

Now if the APC were a valid type of argument this restriction would be unnecessary since it is clear that the meaning of some value terms is taught by means of examples. "How else *could* they be taught?"[8] If, then, this restriction is necessary, the APC is not a valid type of argument. The reason this restriction is said to be necessary is that in applying the APC to value terms, an implicit value premise is involved (and it is the task of the philosopher to make explicit and to consider critically this premise). But even with regard to descriptive terms the use of the APC involves a valuational leap, from ' "X" is applied to t' to ' "X" is correctly applied to t'. There is, moreover, a tacit factual premise involved in going from 't is called X' to 't is X'. For in order for people to call t an X (truthfully) they need only *believe* that t is an X, and in this belief they may be mistaken.

[7] See Antony Flew's approving comments on this point in "Philosophy and Language", *op cit*, pp. 19 f

[8] Cf. Flew, *ibid*, p. 19

Even if there are cases in which the possibility of such a mistaken belief is at a minimum (the case of 'red', for example), such is not the case with philosophical terms. (I come at last to the irrelevance of the APC to philosophical questions.) I shall simply state that there is greater risk of error in the application of philosophical terms than there is in the application of 'red' (which fact is obvious enough anyway), since I should wish to assert that, generally, key philosophical terms are used evaluatively. Hence, on the view of the proponents of the APC themselves, the APC is inapplicable to them. Briefly: some philosophical terms are clearly evaluative, e.g., 'good', 'right', 'beautiful'. Others are seen to have evaluative force on reflection: 'valid' (Urmson's example), 'probable', 'meaningful', 'certain'. That is, the basic terms of ethics, aesthetics, logic, and epistemology are evaluative in character. (We may say crudely that in these disciplines we attempt to set forth criteria of what is good in the way of actions and character, works of art, arguments, and beliefs.) Metaphysics seems to be the exception. But even though it may be doubted that the key metaphysical terms such as 'existence', 'real', 'free will', etc., are evaluative, it is clear that they, like other "descriptive" terms, may be *used* evaluatively. I should suppose that one central reason for the failure to resolve metaphysical disputes is that historically they have been so used, that, e.g., at least from Plato on, metaphysicians have conceived of the real as the valuable. We find evidence of this valuational aspect of metaphysical terms in everyday speech where in the derogatory 'mere' is applied to illusion and appearance, but not to reality, to automatic and habitual actions, but not to deliberate or free ones.

I wish by no means to assert that every philosophical term is used evaluatively. But when we are concerned with the analysis of a non-evaluative term (e.g., 'cause') we do well to see the relation of the term to ones which are evaluative (e.g. 'known', 'meaningful') if we wish to understand why the analysis remains long a topic of dispute, and why (among other reasons) the APC is in such cases of little use.

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DISCUSSION

THE ROLE OF CORROBORATION IN POPPER'S METHODOLOGY

By JOSEPH AGASSI

In his interesting "Critical Notice" on Popper's *Logic of Scientific Discovery* (this *Journal*, Vol. 38 (1959), No. 2, pp. 173-187), Mr. D. Stove discusses almost only one point, Popper's theory of corroboration. He finds in this theory two elements. The first is Popper's emphasis on the significance of the sincerity of the attempts to refute existing scientific hypotheses. This element Mr. Stove considers to be psychological and irrelevant to the problems at hand. The second element is Popper's view according to which a hypothesis is corroborated only when the (sincere) attempts to refute it have resulted in failure. This Mr. Stove considers a rather traditional solution to the problems at hand, and one which may be criticized by traditional arguments, as well as by new arguments which Popper himself states in his book.

I have used the phrase "the problems at hand" twice already; I could not be more specific as I am not quite certain what are the problems which Mr. Stove has in mind. I find it somewhat bewildering that Mr. Stove does not say more explicitly what problems Popper claims to be discussing, what problems Popper's theory of corroboration is intended to solve, or what problems Mr. Stove thinks this theory ought to solve. I wish, therefore, to supplement Mr. Stove's interesting but seriously limited review on these points as best I can. I shall explain why I think that his criticism is entirely valid, and that it is in no way a criticism of the book he was reviewing (except, perhaps, for a criticism of some ambiguity on Popper's part).

1. *Popper's problems and central tenets.* Popper claims to have solved two traditional philosophical problems: the problems of induction (how do we learn from experience?) and of demarcation between science and non-science (by what criterion do we decide which hypothesis is scientific?). His solutions are these: (1) We learn from experience by repeatedly positing explanatory hypotheses and refuting them experimentally, thus approximating the truth by stages. (2) Those hypotheses are scientific which are capable of being tested experimentally, where tests of a hypothesis are attempts to refute it.

(This is the core of Popper's book, as he himself claims,

and as, I should think, is rather obvious anyhow. In this core there is nothing explicit about corroboration, namely about the failure to refute hypotheses; but only of success in refuting some hypotheses, which success is alleged to constitute learning from experience. And Popper's criterion of demarcation does not distinguish between refuted and corroborated hypotheses. By Popper's criterion even hypotheses which were amply refuted by experience are fully entitled to the honorary status of scientific hypotheses.)

Mr. Stove's review relates almost entirely to Popper's theory of corroboration; he devotes two or three rather short paragraphs to the core of the book (pp. 173-174), without relating this core to the theory of corroboration in a clear and explicit manner. Moreover, in these two or three paragraphs he commits some significant errors of presentation.

For example, he attributes to Popper the view that "there is no problem at all about induction" although Popper stresses the opposite, namely that Hume's criticism, which establishes the invalidity of inductive inferences, leads to a few genuine philosophical problems, one of which is the problem of induction, namely, 'how do we learn from experience?'. Mr. Stove, apparently, translates 'how do we learn from experience?' into 'how do we know which of our hypotheses is true or at least probable?'. For Popper this traditional translation is a mistake. Mr. Stove, however, does not notice this, and having attributed the wrong question to Popper, he thereby attributes to him the wrong answer, to wit, the view that a well-corroborated theory is likely to be true.

2. *The traditional view of scientific "success"*. Undoubtedly, the attitude towards Einstein's general theory of relativity changed dramatically with the result of Eddington's observation of the total eclipse, which agreed with the theory rather well. Such a result is viewed by physicists as a great "success", a "verification", a "proof", or "confirmation". Most physicists, I contend, display great pleasure when confronted with "verifications" and give them much publicity, while (with the important modern exception of the discovery of Lee and Yang) the refutation of hypotheses is displeasing to them and is toned down or even pooh-poohed.

If we accept Popper's view, then it seems that we have to view the scientists' traditional delight in "success" and dismissal of "failures" as their misunderstanding of their own activities, or rather as their method of advertisement which need not be taken seriously. And yet, Popper claims, there is an important element in "success".

3. *A descriptive theory of "success".* The empirical support which a theory gains from experience, or its "success", or "confirmation", or "verification", is a measure of its having stood up to severe tests, or, in Popper's terminology, its high degree of corroboration. This is the whole of Popper's doctrine of what "success" is. I wish to stress that the theory according to which "success" is failure to refute a hypothesis does not entail that one "success" leads to another. Nor does it entail that "success" is a Good Thing. Much less does it include the view, sometimes stated explicitly, and more often implicitly, that theoretical science is a Good Thing because it is the body of "successful" theories.

4. *Corroboration and eliminative induction.* Most men of science, and almost all philosophers, think that "success" is a Good Thing, while Popper thinks that "failure" is a Good Thing. Now, many people have stated before that the refutation of errors is a contribution to learning. But they usually agreed that this is only so because some people have preconceived ideas which are mistaken and which are obstacles to learning and therefore have to be refuted; they usually agreed that, had people been cautious and slow to advance hypotheses and to commit themselves to them, the drudgery of refutation could be greatly reduced. Some people, in particular the great though neglected philosopher, William Whewell, claimed that, as it is most unlikely that one should hit upon the true hypothesis before hitting upon false ones, refutation is a necessary preliminary to any discovery of a true law of nature. Yet even Whewell did not think that refutation is good in itself, but rather good as a preliminary which is a useful means for the discovery of the truth. This theory, that we discover the truth through the elimination of errors, is known as the theory of eliminative induction. In some versions of this theory, refuting errors does not necessarily lead to the truth, but, in any case, to hypotheses with high probability or high credibility. In Popper's terminology Whewell's theory can be put (in a somewhat improved fashion) thus: a well-corroborated theory is true, and its corroboration is its verification. The somewhat watered-down version, then, would be this: a corroborated theory is probable or credible.

This last doctrine has often been attributed to Popper.[1] And time and again he has claimed that this is not his view. In Popper's view no degree of corroboration of a hypothesis can secure that it will not be refuted in the next test; no degree of

[1] See von Wright (*Logical Problem of Induction*, second edition, Oxford, 1957), H. G. Alexander (PJPS, Vol. 10 (1959), No. 39, p 234) and S. F. Barker (*Induction and Hypothesis*, Cornell, 1957); this is by no means an exhaustive list.

corroboration of a hypothesis makes it even slightly more probable that it will not be refuted in the next test. Corroboration implies neither verification nor any increase of probability.

To put this somewhat differently, we can neither hope to escape error, nor to make error less likely—not even in a limited field of research. All we can hope is that we shall eliminate some errors, and replace them by other, smaller errors. This is the main point where Popper differs from the eliminationist inductivist. He views as ends what they view as means.

Strangely, Mr. Stove knows this point, but he cannot quite take it seriously. Dismissing this point as inessential to Popper's doctrine, he (rightly) finds Popper's doctrine to be essentially in the eliminationist inductivist tradition; but his dismissal of this point can be explained as his inability to grasp Popper's novel and revolutionary idea.

5. *On the so-called rational degree of belief.* The problem of induction—how do we learn from experience?—Popper has tried to solve *not* by his theory of corroboration but by his theory of gradual approximation to the truth by repeatedly making explanatory hypotheses and refuting them experimentally. Another problem is, how can we avoid teaching false views or at least diminish our liability to assert mistaken views? This question, says Popper, has only one answer: the less you say the less likely you are to err. But science is an attempt to say more and more about the world, so that those engaged in science should have no fear of asserting an erroneous view, but they should do the utmost to encourage criticism. The last traditionally important question is, what theory should we believe, and why? This question, which is central in the inductivist tradition, is hardly discussed by Popper. A glance at the index to his book will reveal how little he says about beliefs, and reading these passages on belief will reveal that most of them are critical of traditional views rather than constructive.

The faith in science is a faith in a certain open-mindedness and detachment of belief—even to the extent of toning down beliefs and viewing them as entirely private affairs. This is the opposite of the view that science tells us what are the true objects of belief, be those the true laws of nature or the most probable among the known hypotheses in the light of the known factual information.

In the beginning of his "Critical Notice" (p. 174) Mr. Stove correctly attributes to Popper the view that empirical

science can lead to disbelief, but not to belief. From the middle of his "Critical Notice" onwards, however, Mr. Stove identifies Popper's view with eliminative inductivism, whose major aim is to prescribe beliefs in probable hypotheses or in verified laws of nature. Take away from eliminative inductivism this prescription of beliefs, and you do get Popper's view. But then the problem arises, how do we gain theoretical knowledge from experience? The eliminative inductivist's answer to this question is, of course, that learning from experience is the same as finding which is the most probable hypothesis—the very answer which Popper rejects. His answer is that we learn from experience by refuting our hypotheses and inventing new explanatory hypotheses, and refuting these again, thus achieving better and better approximations to the truth.

6. *Knowledge and learning.* The problem of induction concerns the question of how we learn from experience, a problem which belongs to the theory of learning, or methodology. Traditionally, however, philosophers have mainly concentrated on the problem of whether or not we have knowledge based on experience, a problem which belongs to the theory of knowledge, or epistemology. There is a good reason for this: if we could show that we have knowledge based on experience, then we could thereby show that indeed we do learn from experience. But all attempts to show that we have knowledge based on experience have so far failed. One of Popper's major revolutionary approaches was to go back from epistemology to methodology. Popper takes for granted (as an empirical fact, if you will) that we do learn from experience, and he asks by which manner, in what way, we learn. It turns out that it is easier to discuss methodological problems than epistemological problems. This is why Popper has so little to say on epistemology though he says quite a lot on methodology.

I am afraid that Mr. Stove has completely missed the point. He claims (pp. 178, 180) that the difference between having invented a hypothesis to explain a set of facts and having discovered these facts as corroborations to that hypothesis is "just nil". In other words, he claims, no matter how we have arrived at our knowledge, it is the state of knowledge that matters. And he says further that discussing this question of how we arrived at our knowledge is a psychological matter. Thus, he claims in effect, there is room only for epistemology and for psychology, but not for methodology, which is outside these two fields.

Now the situation is this. It was Popper who strongly emphasized, in opposition to many inductivist philosophers (in-

cluding Bacon, Newton, and Mill), that from the point of view of assessing our present-day knowledge, the question of how we have arrived at that knowledge is entirely irrelevant. But though epistemologically irrelevant, this question is methodologically highly relevant. Here Popper argues not that it is irrelevant whether or not hypotheses are derived from facts, but rather that it is most often not the case that hypotheses are found by looking at facts; rather they are found by trying to solve concrete problems. This is not psychology. Similarly, though epistemologically it is irrelevant to ask whether we found facts by opening our eyes wide enough, methodologically it is most important to notice that opening one's eyes does not lead to the discovery of new facts, and that it is easier to arrive at new facts by trying to refute our present hypotheses. Psychologists may ask whether we like to refute our pet hypotheses; methodologically what matters is that we learn by doing so. Similarly, from the epistemological viewpoint there is no difference between having discovered facts before or after explaining them. This last assertion contradicts the central doctrine of Whewell. According to Whewell a hypothesis is verified only when it explains a new fact which it was not intended to explain. Now intentions and chronology are irrelevant to the question of whether the hypothesis at hand is true. And the fact that we are often more impressed with corroborations than with explanations is an irrelevant psychological fact. Now that we have agreed (Mr. Stove and myself) that corroboration is irrelevant to the appraisal of our state of knowledge, and that the fact that corroborations are impressive is a psychological irrelevancy, the question to be asked is whether corroboration constitutes progress of knowledge and, if so, why.

7. *Learning by corroboration.* The significance of corroborating evidence is twofold; first, it is in certain respects new evidence, and secondly, it illustrates the high explanatory power of the corroborated hypothesis. I shall not discuss the fact that high explanatory power is considered valuable, because Popper discusses this point at great length. I should only argue that the fact that we pay much attention to a corroborated hypothesis can better be explained by the desire for explanatory power than by the desire for credibility. A corroboration of a refuted hypothesis is pointless from the credibility viewpoint. And yet some corroborations to already refuted hypotheses were very important, because, I suggest, they increased their explanatory power. In this way they rendered it necessary to build future hypotheses in a manner which would yield the refuted but corroborated hypotheses as a first approximation. (This is Bohr's correspondence principle translated into Popper's system.) It is easy to say, after the event,

that the refuted hypothesis was important because it explained much, and not because it was corroborated; the fact remains that we learned that the hypothesis explained much by testing it further and by obtaining corroborations as the results of these tests.

Thus, we want to know not only whether a hypothesis is true or false, but also to find out, as sharply as possible, the limits of its explanatory powers, both from within (corroboration) and from without (refutation).

There is a third argument in praise of corroboration which I put diffidently because, although it is quite simple and may be of some importance, I find it difficult to construct an example for it. Imagine first a development in which a hypothesis A is refuted by the fact a, a hypothesis B which explains both A (as an approximation) and a, but is refuted in the first test by the fact b, and a hypothesis C which explains both B (as an approximation) and b. Imagine a second process, starting with the same hypothesis A and its refutation a, being followed by the hypothesis C which is later corroborated by the fact b. Here, not the corroboration, but the skipping of the stage B, is what makes the second process more rapid than the first. The corroboration is the result of the rapidity, and not *vice versa*.

To conclude, the value of corroboration lies in the discovery of the corroborating facts, in the discovery of as many facts as possible which are explicable by an existing theory, and in its being a result of rapid progress.

8. *Is corroboration really necessary?* So far I have argued that the corroboration of a theory is enlightening—though less enlightening than its refutation—even if it is not really necessary. Mr. Stove, rooted in his inductivist tradition, takes it for granted that factual knowledge which is acquired after inventing a hypothesis could have been acquired beforehand. This is clearly not so with refuting evidence, which is observed as a result of a long chain of deductions of a prediction from a hypothesis. But since, as Mr. Stove observes, the corroboration of a hypothesis is a refutation of a previous alternative to it, perhaps corroborating evidence could be observed before the invention of the hypothesis it corroborates. I do not wish to decide this problem, but merely to point out that while Mr. Stove knows the answer to it as a matter of course, I find it worthy of a critical discussion.

It is a famous idea, already used by Galileo against inductivism, that we see what we think we ought to see, that we interpret facts in the light of theories. This idea, that experience usually tells us only what we have already thought out for our-

selves, makes it particularly difficult to see how we can learn from experience. Both Galileo and Popper have claimed that we can escape this limitation (to some extent) by being extremely critically minded. But Popper, at least, admits that this is easier said than done, and even when we are on the alert we often tend to see things as we expect them to be.

Because of this, Popper suggests, it is preferable to have a number of alternative hypotheses, and to design crucial experiments between them. In this case one hypothesis may be a useful instrument with which to refute another, and in the process of using it we may corroborate it.

There exist striking historical examples to this effect. The deviation of Mercury from the path prescribed to it by Newton's mechanics was not viewed as a refutation of that hypothesis. After all, a few apparent deviations had occurred before, and these were later satisfactorily accounted for without involving overthrow of that hypothesis. Moreover, only after Einstein had explained Mercury's deviation along new lines was a similar, though smaller, deviation discovered in other planets, including Earth, in accord with Einstein's hypothesis. A debate is going on at present as to whether the other two corroborations of Einstein's hypothesis are not in fact refutations of it; I suggest that an alternative to Einstein's hypothesis which would be in better agreement with these facts may alter the widely accepted attitude towards them (especially if that alternative be corroborated). So at least in some instances one may doubt Mr. Stove's assumption that corroborating evidence could be discovered prior to the hypothesis it corroborates, at least in the sense that the evidence is viewed entirely differently when it is a corroboration, in the sense that what might be considered as small deviations or even statistical errors are now viewed as important facts.

But, we should notice, there exist important refutations which were never considered as corroborations. The Michelson-Morley experiment and the Lee and Yang experiments are famous examples of this.

It is obvious, since essentially we cannot predict the result of a test, that we cannot have a satisfactory explanation of corroborations in general. Nevertheless, I wish to express my profound sense of puzzlement at the incredible corroborations which some theories have received, in the past and in our own times. I cannot escape the feeling that it is as if a deity paid us a premium for any good explanation and let it be well corroborated before it be refuted. To use the inductivist language, I am willing to bet at any odds that if a reasonably good solution should be found to any of the major problems in contemporary physics, it

would be well corroborated within a short time, and with much greater ease than it would be later refuted. I cannot support this feeling, which, of course, may be totally pointless. And I do not think that Popper's philosophy so far explains the amazing fact, if it is a fact, that so much corroboration is to be found in the history of science, although, I think, Popper's view is the only reasonable explanation of the value of corroboration.

But, I should add here, in a recent public lecture[2] Popper has claimed that corroboration is important, apart from being enlightening as discovery and increasing the explanatory power of a hypothesis, and is even essential to science—as encouragement to our research. I confess that I do not quite see this point, and I would here join Mr. Stove in asking what is the essential difference between corroboration and explanation. If psychological factors are ignored, and if the corroborating facts would have been found before the hypothesis which they corroborate, why, then, is corroboration essential?

9. *The indispensability of the corroboration of factual reports.* As Popper has pointed out, one kind of corroboration is essential for science. It is the corroboration of factual evidence, or rather of its spatio-temporal universalization, which is often called "a general fact" or "an observable" or "a generalization". We know that only repeatable facts are considered in science, and repeatable in the sense of corroborable, not in the sense in which the reports about flying saucers are constantly flowing in. The reason for this is that a fact is important when it contradicts a theory. And it is important to have them corroborated in order to make it simpler to accept the reports as those of general facts rather than to explain them away in some *ad hoc* fashion.

Popper has no wish to explain the fact that the generalizations we propose are often corroborated: it is the task of specific scientific hypotheses to explain specific generalizations which are corroborated (and thereby to explain the fact that they are corroborated). This would not satisfy an inductivist, since such specific explanations are dubious; he wants certitude or at least high probability. But he is asking for the impossible. All one can say in a general manner is that if we had had no corroborable general facts we would not have science as we know it. But a world with no general facts is, perhaps, one in which even life is impossible.

10. *Conclusion.* Mr. Stove takes it for granted that Popper's

[2] 'The Idea of Truth and the Problem of the Empirical Character of Scientific Theories', delivered in the International Congress for the Philosophy of Science, at Stanford, California, in August. 1960.

theory of testing is a preliminary to his theory of corroboration, and that this latter theory is a solution to the problems: how do we know and what should we believe? But Popper tries to solve the problem: how do we learn? His answer is: by criticizing our errors. The idea that anything we say can be a subject for a critical examination is the core of Popper's philosophical attitude. Mr. Stove views Popper's recommendation of the critical attitude as a part of his theory of corroboration, and he tries to see whether it is a necessary or an eliminable part of it. He is thus putting the cart before the horse. Popper takes the critical attitude as fundamental. Corroboration according to him, is one sort of happening in the history of science which results from this attitude and to which, in turn, this attitude should be applied. Mr. Stove takes it for granted that, to Popper, a corroborated theory is corroborated because it is true or likely to be true or credible. As I understand it, Popper's philosophy contains the idea that we should take notice of a well-corroborated theory and try to explain the fact that it was corroborated—and a variety of explanations may be available, each of which should be critically examined. Undoubtedly, Popper's philosophy is connected with a long-standing tradition; but it is the critical tradition of Galileo and Boyle, of Kant and Whewell, rather than the inductivist tradition of Bacon, Newton, and Mill.

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THREE QUESTIONS FOR PRIOR ON TIME

By JOHN KING-FARLOW

Professor A. N. Prior's views on Time are as thought-provoking as anyone's since McTaggart claimed to unravel the contradictions of temporal talk. To be provoked, however, is not to be convinced and I should like to add three questions to criticisms already advanced over *Time and Modality*.

(I) *Would Prior's ambition eliminate Determinism?* Prior begins his book by confessing to 'a hankering for well constructed theories which much contemporary philosophy fails to satisfy".[1] The kind of well constructed theory he has centrally in mind is a logical system in which tense operators perform like Lukasiewicz's modal operators in such a way that, together with certain rules of inference, axioms and truth values, various metaphysical proclivities of Prior's are satisfied. His main concern is

[1] *T & M.*, preface, p. vii.

to bend logic to "bring out the logical asymmetry between past and future which serious indeterminism seems to demand".[2] He would formalise the view "that from the *fact* that there is a sea-battle going on it does not follow that there was *going to be one,* though it does follow that there *will have been one*".[3]

Thus Prior's first and intensest motivation for adventuring with symbols is his desire that logic should prejudge the determinist-indeterminist issue. Commenting on this desire, L. J. Cohen has written: "Ordinary logic with its timeless truth evaluations is quite uncommitted to either side. Neither 'Any point instant belonging to a day subsequent to the present one is necessarily occupied by whatever occupies it', nor its contradictory, is a thesis of any familiar text book system. If tense logic tends to beg the question in favour of indeterminism, that is hardly a reason for calling it 'good logic' even if it is good physics or metaphysics".[4] In one way Cohen's criticism goes too far and in another way not nearly far enough. If a metaphysician so bends logic or language as to make it reflect his metaphysical bias, then the finished article *is,* by his canons, a good logic or a good language. But Prior is not merely mistaken in assuming that tense logic is a *necessary condition* for satisfying the demands of serious indeterminism. He is further mistaken in thinking that setting up such a logic supplies anything like a *sufficient condition.*

As to determinism, in any of the usual senses, I certainly agree with Cohen, Donald Williams[5] and others that standard quantification theories are agreeably neutral. On the other hand, whether or not we believe in the existence of timeless or temporal truths about the future is not a question the negative answer to which qualifies us to be called indeterminists, in the appropriate sense of defenders of free will. (That sense of indeterminism is presumably what Prior's references to Aristotle's sea-battle indicate.[6]) Suppose, like C. D. Broad in *Scientific Thought,* I refuse to call any proposition true, false, true-or-false, or even a proposition unless it corresponds or fails to correspond to a suitable past or present fact. This affords me a magnificent asymmetry of Space and Time, perhaps, yet it does not prevent my believing that there is no such thing as human choice, that all present and past facts of human behaviour show distressing regularities no less mechanical than those of brute beasts or falling bodies. It might be suggested that restricting the indeterminate value to

[2] P. 94.
[3] P. 95.
[4] *Philosophical Quarterly,* 1958, p 270.
[5] *Journal of Philosophy,* 1951, pp. 457 ff.
[6] Cf. *De Interpretatione,* IX.

future propositions about human behaviour is what serious indeterminism demands. This ill fits Prior's willingness to forsake the existence of "facts directly about" *any* objects which do not yet exist. We might move a shade closer to reflecting indeterminism if there were an asymmetry between reference to human and to non-human future objects, but Prior does not provide this. Even so a neuter value concerning future human behaviour could equally well reflect belief (a) that man is utterly unpredictable (hence *not* free, cf. Hume) or (b) that the continued existence of men is too precarious for one to be committed to any future assertion about them. So my first question is: why does tense logic reflect "serious indeterminism" any more than determinism is enforced by timeless truths?

(II) *Do Prior's justifying remarks about reference to what is future support or merely follow from his metaphysical view of Time?* In his (seventh) chapter on Common Noun Logic and at earlier stages Prior is magnanimous about dispensing with the reference resources we have in ordinary language and quantification theory. This is in spite of his campaign promises to respect some basic intuitions of the former and not to tamper with the latter. He writes in self-justification at an early stage: "I am a little uncomfortable about this view that we cannot properly name objects which have ceased to exist, like Bucephalus; but I do not see any way of avoiding it—if we say that we *can* properly name them . . . we are exposed to all the difficulties which were shown earlier to arise with the general theory that there are non-existent objects. Instinctively, all the same, we are happier about granting that we cannot properly name, and there are no facts directly about, objects which *do not yet* exist".[7] Prior goes on to invoke in his support the "very powerful" arguments which Professor Gilbert Ryle raised in *Dilemmas* against naming or referring to the non-existent.[8]

Turning to these "very powerful arguments" of Ryle's, we meet a rather odd inference pattern heavily coated with bluff.[9] We are offered the obvious premise that we "can never point to or name a particular happening and say of it 'This happening was averted' "—obvious, that is, in the sense that talking *about* what did not happen is not entirely like talking *about* what did happen. In the same way, Ryle says plausibly enough, if the Waterloo of 1814 had not been fought or the present Ryle not been born, then there would be no such event or person for historians to describe: certainly there is a sense of *describe*

[7] *T. & M.*, p. 33.
[8] Pp. 33-34.
[9] *v. Dilemmas*, pp. 24-27.

appropriate to Ryle's claim. However, Ryle moves on confidently to put *what has not yet occurred* on a logical par with *what did not occur* at some specified past time: in other words we are expected to bracket what is not yet the case with what now can never be the case, as if they were of the same order of substantiality. Why the latter should be so like the former from a referential point of view we are never told. Why indeed should the potential and perhaps all but certain stand in the same sort of relation to the referrer as the now utterly impossible non-entity? The metaphysical view which would lead us to hold this is going to involve Broad's asymmetry of Space and Time, whereby to be present is "simply to precede *nothing*". Ryle like Broad insists on a radical difference between prophecies and chronicles. There is a Broadian ring about Ryle's assertion that no prophet, however vivid and accurate, could "get the future events themselves for the heroes and heroines of his story". Ryle talks throughout as if an examination of our common-sense ways of talking makes it impossible to differ with him, but in the case of this last extravagant metaphor no appropriate common-sensible sense of *get* is clear.

It is this crude metaphor of *getting* which more than anything else suggests that Ryle has presupposed Broad's by no means universally acceptable picture of History. Here a pile of hard specious-present-sized events—which, having already become, we can really get hold of to refer to—are opposed to the practically ineffable vacuum of what is not yet. Similarly, Prior in his paper 'Time after Time'[10] baulks at Pears' talk of the logician's truths as timeless shadows set in a symmetrical Heaven. He prefers to think of events as casting their shadows over what will have become after them, shadows that lengthen with the passage of time. On a rival metaphysical view, like that of Williams, who views the totality of events as spread out *sub specie aeternitatis* in the dimensions of Space and Time alike, we get a symmetry which would make us want to interpret matters of reference very differently. An odder view might make us want to restrict the possibility of *direct* reference to things present and future. How ordinary language could help us to decide between such rival metaphysicians and their accounts of reference is obscure; certainly Ryle and Prior make it all no clearer. So my second question runs: are not Prior's and Ryle's parsimonies in the matter of future reference merely consequences of, not justifying reasons for, their questionable metaphysics of Time?

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[10] *Mind*, 1958, pp. 244-46.

(III) *In so far as Time can be made logically special, cannot Space and Individuals be made special, too?* In a long section of appendix Prior tries to show that there is *something special* about Time.[11] Constructing some ingenious place-logical and time-logical formulae he argues that we can equate an expression meaning "it is the case *m* miles to the left that it is the case *n* miles to the left that *f*" with another expression in which a symbol represents the algebraic sum of *m* and *n* or, with a wide range of directions, the vector sum. But, he insists, there is no such analogy in the case of time-logical formulae: "for even if it was the case *m* days ago that *p*, it might not have been true *m* plus *n* days ago that it was going to be the case *n* days later that *p*. For *m* plus *n* days ago the issue might have been indeterminate". No auxiliary symbol parallel to the one in place-logic can obviate this.[12]

We have already rejected the thesis that we must bring in a neuter value to save future contingencies: Prior's alleged *need* for special values relative to Time is specious. A neuter value in the case of futures yields not Indeterminism, but something like the unreality of the future, a very different metaphysical position. Williams has retorted to Broad that we might equally well posit the tragic unreality of the past. This view could equally well be represented—not *proven*—in Prior's Time-logic by assigning neuter values to all formulae about the past. Again, a man walking ever forwards along a straight line might equally well believe in the utter nothingness of all he had left behind. In a backwards-forwards logic he could represent—not prove—his thesis by assigning the special neuter value to all formulae about the regions behind. Again, Prior tells us, though he does not argue in such detail, that there is something special about times as opposed to individuals. But a Platonist could represent the ontological hierarchy of the Line parable in *Republic* Book VI by assigning an ascending order of truth-values to formulae mentioning the ascending order of *gignomena* and *onta*. So my final question runs: surely the only restriction on making times, places or individuals logically special is that we must not make them *all* special at once, or else none of them will be special?

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[11] *T. & M.*, pp. 117-121.
[12] *v.* pp. 119-120.

A POUND IS A POUND IS A POUND

By RONALD J. BUTLER

The logical tangle of the theory of Forms is sometimes met by the suggestion that the theory is to be conveyed not by precise description, but by analogies and allegories and equivocal meanings. It is in this spirit that Mr. Geach has suggested that the Forms are paradigms, comparable to standard weights and measures.[1] The suggestion warrants a hearing not because the logically incomprehensible can become less incomprehensible by a spate of analogies and allegories and equivocations, but rather because the incomprehensibility of the Forms can be thrown into sharper focus by showing the respects in which the analogy fails.

The analogy was introduced in part to elucidate what Professor Vlastos had called the *self-predication assumption* in the Third Man Argument. Although I do not agree that Plato there tacitly assumed that every Form is an example of itself, at least it can be agreed that Plato at some time thought certain Forms to be examples of themselves. The only ones he explicitly mentions are the Just, the Holy and the Beautiful. There is indeed a class of Forms which must have this character, namely the class of Forms of whatever is characteristic of the Forms. I think it not implausible that the three clear-cut cases which Plato mentions were all thought by him, at one time or another, to be members of this class. For example, since all of the Forms are divine and immortal (*Phaedo* 81a), and the ultimate goal is to contemplate the Forms eternally, what else could be holy if the Holy is not holy? And again, the culmination of Diotima's speech is that the beauty in every Form is one and the same (*Symposium* 210ab, 212a), so that as a matter of course the Beautiful is itself beautiful.

Geach's analogy, however, was introduced not to elucidate the Forms of characters, but rather the Forms of objects. In point of fact Plato never gives a clear-cut case of the Form of an object as an example of itself. He does not *say*, nor does he *imply*, that the Bed is a bed,[2] or that the Circle is a circle. Many passages have been cited in support of the contention that Plato thought of Forms other than those explicitly mentioned as being examples of themselves; but I find none of these other

[1] P. T. Geach, "The Third Man Again", *The Philosophical Review*, Vol. 65 (1956), p. 74. All page references in the text are to this article and to Professor Vlastos's reply, "Postscript to the Third Man", in the same issue of *The Philosophical Review*.

[2] An analysis of Plato's argument at *Republic* 597c is contained in "The Measure and Weight of the Third Man", to be published in *Mind*.

alleged cases at all convincing. There is some doubt, therefore, as to whether the analogy has any application—so far as "the self-predication assumption" is concerned.

Yet Geach means the analogy to elucidate not only this assumption, but also the relation of other things which are not Forms to the Forms in which they participate. "The bed in my bedroom is to the Bed, not as a thing to an attribute or characteristic, but rather as a pound weight or yard measure in a shop to the standard pound or yard" (p. 74). The analogy must therefore be examined in the light of both claims.

Initially Geach is doubtful as to whether "The standard pound weighs a pound" is admissible at all. On the ground that one cannot measure a standard against itself, he is *tempted* to say, but does not say, that such statements are plain absurdities (p. 74). This double line is developed in the penultimate paragraph, for there he says that the standard pound weighs a pound no matter what it weighs, only to add that "just because there is a sense in which the standard pound must be a pound, there is a sense in which it cannot be a pound—not as other weights can be" (pp. 81-82). At this point it is said that one can predicate ". . . is a pound" of the standard pound and the many pounds only analogously: to which it must be replied that the use of "analogous" in regard to meanings seems to have admitted of no precise explication.

In the quest for precision Vlastos seeks instead the exact sense in which the one use of ". . . is a pound" is derivative from the other. The standard pound is said "to have the same weight as that of objects which have been (or, can be) found to weigh the same as the Standard Pound" (p. 88). It is on account of this comparison with things that weigh a pound in the ordinary sense of weighing the same as the standard pound, that the standard pound itself is said to weigh a pound derivatively.

Suppose that Mrs. John Doe is handed a pat of butter and asked how much it weighs. After some deliberation she says, "It has the same weight as that of objects which have been (or, can be) found to weigh the same as the standard pound". Would she not be saying, in her tortuous way, that this pat of butter *weighs a pound*? Suppose instead that Mrs. Doe is handed the object which is said to be the standard pound: could she not give precisely the same answer? This remains true in cases wherein one fails to recognise the object as the standard pound, and in cases wherein it is known to be the standard pound. The latter situation may seldom if ever arise, and if it did one would be inclined to say that of course *that* object weighs a pound. The question "Does that object weigh a pound?" when it just happens

to be the standard pound is not felt to be at all odd: it can in principle be asked of any object which one might lay hands upon. One might even say, "This object weighs a pound, and it might even be the standard pound. Perhaps the Scottish Nationalists left it here for us to find". The other question, "Does the standard pound weigh a pound?" is felt to be more odd, not because it is wrong, but because when the object is specified as the standard pound the question requires a special context. When given that context the context seems odd because it is so precious: but the question in that context no longer rings oddly.

Geach's initial qualms arose over the notion that you cannot measure a standard *against* itself. In every case except one, to say that something is a pound might be taken to mean that *if* the object in question were weighed against the standard pound, then it would be found to weigh the same as the standard pound. But it is a mistake to think that the object which is called the standard pound cannot satisfy the condition laid down in the antecedent simply because it is the standard pound. The reason why the condition laid down in the antecedent cannot be satisfied is because the relation of *weighing against* is irreflexive. "If the pat of butter from Minnesota is weighed against the pat of butter I bought yesterday, it will be found to weigh the same as the pat of butter I bought yesterday" is a perfectly good sentence, provided that the pat of butter I bought yesterday is *not* the pat of butter from Minnesota.

The expression ". . . weighs a pound" can be explicated in terms of the relation of *weighing the same as,* without reference to the relation *weighing against.* Imagine a community which did not use the principle of the balance as the immediate and obvious means of measuring weight, but which had at its disposal other means for telling when two objects weighed the same, or when an object weighed the same on different occasions: then the difficulty of weighing the standard pound against itself would not arise. That is to say, the notion of *weighing against* is dispensable in a way in which the notion of *weighing the same as* is not.

Suppose that the object referred to as the standard pound—let us call it the *SP* in order to distinguish it from the concept of the standard pound—has been kept for a long time in a glass case at a constant temperature. When taken out and tested it is found no longer to weigh a precise pound, there being a uniform discrepancy in all the tests. Imagine the flurry in the world of science, the panic in the world of commerce, the problems in the world of legal enactment. But none of this would cause the standard pound to be abandoned: too much is at stake for there to be any immediate change. True, the *SP* would be abandoned,

since it would no longer accurately represent the standard pound; and to replace the late *SP* would be an engineering problem of the first magnitude. In general, however, standard weights and measures do not exist in isolation: the standard pound is so many grammes, the standard metre is so many inches, the standard litre of H_2O weighs so many pounds and no more. Standard weights and measures and volumes dovetail together so tightly that it is not true to say that the *SP* weighs a pound no matter what it weighs. The coherence of standards stabilises the standard pound far more than does the *SP,* which could at any moment be detonated out of existence. By virtue of the dovetailing of quite different standards, the phrase "the standard pound" has liaisons with other expressions in the language of such a kind that we can readily verify if the *SP* truly represents the standard pound.

The *SP* has weight, but things having logical liaisons have no weight. To say that the standard pound *weighs* a pound is as absurd as to say that the number three weighs a pound, or that the number three is three things. But that the standard pound *is* a pound is as tautologous as that the number three is a number. Spelled out thus it sounds a little strained: it is far more usual to say without ado that three is a number, or that a pound is a pound.

Could the relation between the things in the world of Becoming and the Forms be like that between the *SP* and the standard pound? If this were so it would need to be assumed that the Forms are conceptual in the way in which the standard pound is conceptual, instead of being things of beauty and objects of reverence. In raising questions about whether the Forms are purely conceptual and can be predicated of themselves a confusion has arisen between "the Form, the Master" (or, more simply, "the Master") which is a referring expression and the Master which, being a Form, does not refer. We can ask of the former whether it can be predicated of itself, but in the case of the latter this question is inapposite. And we can ask of the latter whether it is an example of itself, but in the case of the former this question is inapposite. The standard pound is determined by the manner in which it is specified in statutes, rulings, legal laws and scientific laws; but the Master is not and could not be determined in this manner. Whatever the Master be, it was originally conceived by Plato as a non-temporal and immutable entity in the world of Being. In contrast the Bureau of Standards strives for consistency in meticulously checking standards in the world of Becoming which are intended to endure: but bureaucrats know only too well that the standards could for expediency be changed.

Wherein lies the merit of Geach's analogy? Its value, in so

far as it has any value, lies in focusing attention upon the ideal or unique character of the Forms, upon the claim that the things around us do not measure up to their Forms. It becomes devalued upon discovering that there may be two distinguishable things, not one, that both of these things could change, and that the precise relation between ordinary pounds and either of these things throws no light at all upon the precise relation between a bed and the Bed. So far as "the self-predication assumption" is concerned, it must be reiterated firmly that the Forms, like the *SP*, not being expressions, are not self-predicating expressions.

It is clear that when Plato said that the Beautiful is beautiful he was intending to make a non-tautological statement about an entity in the world of Being. In contrast, the concept of the standard pound, while being non-temporal, can be predicated of itself only tautologically. And the object called the *SP* can be regarded as an example of itself in that any pound weighs a pound; but this, while being non-tautological, has been shown to be not the appropriate parallel.

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REVIEW

THE LOGIC OF SOCIAL ENQUIRY. By Quentin Gibson. London, Routledge and Kegan Paul, 1960. 214 p. 24s. (U.K.).

Much of the discussion and controversy concerning what it is that social enquirers are doing is frequently muddied by cross-purposes, *ignoratio elenchi,* concealed ambiguity, and failure to make explicit the presuppositions made by the participants. Much of it is sterile because of the failure to disentangle separate issues or, where they have been disentangled, to be as precise as possible in defining them. Professor Gibson's book is concerned with disentangling and precisely defining the issues in the controversy about the nature of social enquiry. But, further, it defends a view of social enquiry as scientific and gives some account of the nature of social enquiries as systematic bodies of knowledge, based on a "methodological individualism".

"The controversy", he says (p. 1), "has arisen . . . out of reflection on differing presuppositions about method which have been implied or thought to be implied in the conduct of these enquiries and out of the differing expectations which ordinary people have about the character and extent of their results". The book is in two parts. Part One (Chs. I-VII) is entitled "Anti-Scientific Views about Social Enquiry" and it examines some of these main differing presuppositions about method. Part Two (Chs. VIII-XVI) is entitled "The Logical Peculiarities of Social Enquiry" and it examines the character and extent of the results that can be expected from social enquiry. The material is very well arranged, and the book is a sustained and coherent argument. I repeatedly found that what I had noted as apparent deficiencies in the argument in early chapters were taken up and fully discussed later in appropriate places.

In Part One a set of claims by which it has been thought to show that social enquiries cannot satisfy one or other of the conditions necessary for any enquiry to be scientific is examined. There have been quite crude attempts to deny the scientific character of social enquiries, as, for example, when a science has been narrowly defined as any enquiry employing the technical procedures of physics. Such as these have not been discussed, and justifiably so, since so large a failure to accord with historical usage would rule out much more than social enquiry. Instead, the proposal (p. 3) is to adopt the definition of a scientific procedure as any procedure that employs each of the following: abstraction, generality, reliance on empirical evidence, ethical neutrality, and objectivity. The argument of the first part is that social enquiries

are not, through any peculiarities either of the subject-matter or the enquirers themselves, unable properly to employ such procedures.

Claims in writings on social theory about the uniqueness of social phenomena, their variety, and the rapidity of social change, about the inefficacy of empirical procedures, the ultimate impossibility of positive, factual, value-free conclusions, and the impossibility of generalising are frequently made in the most tantalising of obscure ways. Chapters II-VI provide a very clear statement of such issues and, if heeded, should do much to dispel some of the woolliness that has hindered discussion of the difficult topic of the status of social enquiry. It is not likely that the book will be regarded as resolving any of the crucial issues, since these are, or are closely related to, matters of continuing philosophical dispute. It has the virtue, however, of showing what issues are worth discussing, and of discussing them in a careful and detailed way.

There are many matters of general philosophical interest raised. In the two chapters on "The Criticism of Generality" (Ch. III, ". . . Freedom and Change", and Ch. IV," ". . . Purposes and Reasons"), where the arguments against the use of general statements in social enquiry are examined, such topics as free-will, motives, reasons, purposes and intentions are discussed as providing possible explanations of social affairs which can avoid generalisation. It would be an unreasonable overburdening of the book to try to argue out in detail each of these, although the author makes his own position clear and succeeds in bringing out many of the confusions that there are in discussions of them. For most of these topics, the discussion is appropriate, but occasionally a little more exposition and discussion would have helped. In his discussion of indeterminism, for example, he says (f.n., p. 22) that "it is impossible to enter into the subtleties of this controversy here". It is likely, however, that anyone who wanted to urge the occurrence of free-will against the possibility of general explanations in human affairs would want to enter into the subtleties of the argument. Such a person, too, would argue that the social psychologist's speculations in the example on p. 23, which "might lead him to apply to this case not only rules of chance but universal laws of human behaviour", would be utterly idle. The argument from free-will is so frequently urged in this context that a more detailed statement of this issue, and a more explicit justification of the author's own position, would have been an advantage.

Objectivity has a special place in the list of defining characteristics already mentioned. According to the argument of the

book, the upholders of "anti-scientific views" can admit that social enquirers do engage in the first four activities. They occur in social enquiries, but they are ineffective: there are effective, alternative procedures, and it is only by adopting these that important knowledge of human and social affairs can be established (pp. 3, 74). "But in the case of objectivity the position is reversed. They do not wish to criticize a procedure in which objectivity is maintained—they wish rather to deny that it is possible. They recognize that it is desirable for social enquirers to take account of evidence, but they assert that the general circumstances of social enquiry are such as to prevent even the most intelligent from using the evidence at their disposal" (p. 74).

Objectivity is important also in Gibson's argument. In asking whether social enquiry is objective, he explicitly considers the theorist. He considers the question, 'Can social enquirers be objective?". He could also have explicitly considered the theory. Social theories are sets of statements, standing in specific logical relations to one another. It is possible to ask whether such theories can be objective and mean by this something quite different from asking whether theorists are objective. What one would be asking about would be, for example, such matters as the kinds of statements that occur in the theory, the possible evidence for them and its accessibility, and the logical relations that hold among them. These matters are discussed in the book, but not directly in relation to objectivity. The topic of the kinds of statements that are possible in social theory and of their logical relations is one that receives considerable attention. The most important account of these is that of "tendency statements" and their function in systematic theory (Chapter XIII). Other statements considered are ethical or value statements and those statements asserting the occurrence of psychological experiences or states of mind.

Even if it were the case that social scientists cannot avoid value-judgments, by which I mean the making of ethical statements, rather than being influenced by ethical considerations in arriving at conclusions or in considering evidence (about both of which Gibson is extremely lucid), only certain ethical theories pose problems for the discussion of objectivity. These are those subjectivist theories that claim ethical terms to be reducible to terms describing subjective feelings or states of mind, and those that claim ethical terms to be irreducible. Theories of the second kind pose the problem of whether certain propositions of social theory require methods of observation other than regular empirical methods. Theories of the first kind, along with those other state-

ments asserting subjective experiences, raise questions concerning the accessibility of evidence.

On the last of these Gibson supports introspection and denies the behaviourist and Rylean account (p. 48 and f.n. 1), i.e., he claims that the empirical evidence about the states of mind of people other than ourselves cannot be anything but indirect. He argues (p. 49) in support that "the position of the social enquirer is in this respect similar to that of other enquirers" and instances the evidence for past events and the evidence for theoretical entities, such as "light-waves, electrons or genes". Physicists and biologists, he says (p. 49, f.n. 1) leave the interpretation of these to philosophers "and there is no reason why social scientists should not do the same".

Now, there is an important difference of principle here, namely that, in the case of the theoretical entities of physics and biology, no one is in principle in a privileged position in relation to the evidence, whereas, in the case of states of mind, it is claimed that in the last resort someone has privileged access to some of the evidence. Indeed, he says (p. 49) that "the empirical knowledge we have of the thoughts and feelings of others depends, in short, on generalization from our own experience". The difficulties of such generalization are not underestimated, but it seems to me that some of the arguments in Chapter V against "sympathetic understanding" and "intuitive insight" as effective, alternative procedures to empirical investigation might apply equally well to the *ultimately* private nature of the evidence for statements about a person's states of mind.

An objective procedure is described (p. 3) as one "in which no-one is prevented by the general circumstances of the enquiry from basing his statements on a consideration of the evidence". The denial of the possibility of objectivity in social enquiry is the contrary of this, that anyone is prevented by the general circumstances of such enquiry from basing his statements on a consideration of the evidence. The circumstances considered relevant are those of motives, custom, and social situation (p. 77 ff.). The discussion of these and related points in Chapter VII, "The Denial of Objectivity", is very good, but one part of the argument (which is important, because it is taken up and developed in Chapter XIV, "The Assumption of Rationality") seems to be deficient in not examining the notion of "evidence" sufficiently.

The following is a summary of the argument of Chapter VII, pp. 73-76. To deny that a man is objective is to assert that he is prejudiced, biased, protecting special interests and the like. It is a form of *argumentum ad hominem,* and is essentially a

means of attacking someone for the way in which he comes to hold his beliefs. When it is directed against the whole body of social enquirers, it is a means of attacking social enquirers as such for the ways in which they come to hold their beliefs. Depending as it does, then, upon causal considerations or considerations of origin, it has been, normally implicitly, taken to be irrelevant to the *logic* of social enquiry, where the important questions are those of beliefs and the evidence for them. It is not, however, irrelevant for this reason: that in relation to beliefs, evidence has a dual role. It is at once the logical justification of the belief, and the cause, or part of the cause, of the belief, at least in those cases where the belief is in some degree rational. For this reason, the possibility of objectivity, the possibility of arriving at beliefs, not under the influence of custom or one's social situation, but by taking account of the evidence, needs to be shown.

I agree with the conclusions arrived at concerning the possibility of objectivity of enquirers, but think that their support requires more emphasis upon controversy, mentioned only in the last paragraph: in other words, upon the conditions of dispute, challenge, and, especially, the possibility of *agreement* among social enquirers. In this context, objectivity or the lack of it would be demonstrated in such things as meeting requests for elucidation of claims, for considering suggested counter-evidence and so on. Relevant to this would be the principles of explication, the principles used in assessing evidence and in determining what is to be admitted as evidence.

I should have liked to see in the book some general discussion of the admissibility of evidence in social enquiry. He has much to say about evidence, but it seems to me that he is not perfectly clear about two quite different kinds of thing. When he discusses the evidence for beliefs, he speaks sometimes of evidence in an absolute, non-rational or non-logical sense, as, roughly, what there is to be discovered. It is in this sense that evidence is used in the discussion of the denial of objectivity. Showing that *the* evidence can be taken account of is showing that what is going on in society can be discovered. The upholder of the anti-scientific view denies that what is going on in society can be discovered directly and says that it can only ever be observed indirectly through some distorting set of concepts. To deal properly with this would require, however, a general discussion of evidence and Gibson has explicitly restricted himself so far as possible to questions of scientific method in social enquiry.

Evidence in the rational or logical sense consists in propositions standing in certain logical relations to other propositions

and having a certain status among enquirers, namely of being accepted (or believed) in one sense without evidence. When, for example, a social theorist is charged with bias, i.e., with not having considered the evidence, he may meet the charge by arguing that "the evidence" is not really evidence. I should have liked to see some amplification of this sort of issue in the book. This, however, may be quibbling.

There are many other topics of importance in the book that I have not mentioned. Among these are very good discussions of the nature and function of general statements in social enquiry, historical explanations, and the relation between psychology and social enquiry. There can be no doubt that the book is an important contribution to the discussion of matters that do not often receive the careful and rigorous attention that Gibson gives them.

I have noted only a few minor misprints that do not affect the argument.

R. S. WALTERS.

BOOKS RECEIVED

(Inclusion in this list neither guarantees nor precludes later review.)

ARMSTRONG, D. M. Berkeley's theory of vision; a critical examination of Bishop Berkeley's *Essay towards a new theory of vision*. Melbourne University Press, 1961. xi, 106 p. 26s. (Australian).

Argues that Berkeley's *Essay* is more important as "a heterodox view of the general structure of the reality revealed to the senses of sight and touch" than as a half-way house to immaterialism. The author is, however, critical of Berkeley's views on the relation between sight and touch.

BACON, Francis. The new organon, and related writings; edited, with an introduction, by Fulton H. Anderson. New York, Liberal Arts Press, 1960. xli, 292 p. $1.35 (paper covers).

BAHM, Archie J. Types of intuition. (University of New Mexico Publications in Social Sciences and Philosophy, number 3.) Albuquerque University of New Mexico Press, 1961. $1.25 (paper covers).

"The present study demonstrates how intuition is presupposed in all knowing . . . introduces the theory of knowledge of organicism, a new integrative type of philosophy." Blurb.

BLANCO, Alejandro Diez. La filosofia y sus problemas. Barcelona, Editorial Scientia 1960. 217 p. 200 pesetas.

A critical exposition, for beginners, of leading philosophical theories. Deals with epistemology, philosophy of science, ontology, philosophy of religion, and ethics.

CRAIG, Hardin. New lamps for old. Oxford, Blackwell, 1960. viii, 244 p. 4s. 6d. (Australian).

Rejecting positivism, the author tries to find in "the philosophy of relativity with its naturalistic theory of cognition" the germs of "a new renaissance" in "humanistic knowledge and humanistic relativity".

DATE, V. H., ed. Vedānta explained: Samkara's commentary on the Brahmasūtras, volume 2; with, New light on the philosophy of Samkara. Bombay, Bookseller's Publishing Co., 1959. viii, 552 p. Rs. 27.50.

EDEL, Abraham. Science and the structure of ethics. (International Encyclopedia of Unified Science, vol. 2, No. 3.) University of Chicago Press, 1961. iv, 101 p. $2.25.

Discusses scientific method in ethics, and concludes that using it is "genuinely possible, with the extent of its practicability dependent on the extent of determinateness found in the human field".

FEIBLEMAN, James K. An introduction to Peirce's philosophy. London, Allen & Unwin, 1960. xx, 503 p. 50s. (U.K.).

"This book has two aims . . . to offer an introduction to the general philosophy of Charles S. Peirce . . . [and] to exhibit the system which seems to be inherent in Peirce's philosophy."—Preface.

GARNETT, A. Campbell. Contemporary thought and the return to religion. Lexington, Kentucky, College of the Bible, 1960. 99 p. $2 (paper covers).

GENTILE, Giovanni. Genesis and structure of society; translated by H. S. Harris. Urbana, University of Illinois Press, 1960. 228 p. $4.50.

Gentile's last book, written in 1943 and posthumously published.

GILLESPIE, Charles Coulston. The edge of objectivity; an essay in the history of scientific ideas. Princeton University Press; Oxford University Press, 1960. x, 562 p. 82s. 6d. (Australian).

An interpretative history of scientific thought from Galileo to Maxwell.

HALLDEN, Soren. True love, true humour and true religion: a semantic study. (Library of Theoria, No. 6.) Lund, Gleerup; Copenhagen, Munksgaard, 1960. 112 p. 13 Sw. Kr.

Discusses what is meant by talk of essences ("the essence of religion" etc.) using these three terms as examples.

HARRIS, H. S. The social philosophy of Giovanni Gentile. Urbana, University of Illinois Press, 1960. xii, 387 p. $5.75.

"The aim of the present study is to set aside the polemical prejudices arising from Gentile's commitment to a particular political regime, and to consider this theory on its merits as a theory . . . A great gulf always existed between the theory of 'Fascist idealism' and the actual practice of Fascism . . . and . . . 'Fascist idealism' is itself a radical deformation of Gentile's theory."—Preface.

HEIDEGGER, Martin. Essays in metaphysics: identity and difference. New York, Philosophical Library, 1960. 82 p. $2.75.

Two lectures published in German in 1957.

KIMPEL, Ben. The principles of moral philosophy. xviii, 234 p. New York, Philosophical Library, 1960. $3.75.

An attempt at what the author calls "an empirical moral philosophy"; by which he seems to mean one that will help us to realise worthy moral ideals.

MINKUS, P. A. Philosophy of the person. Oxford, Blackwell, 1960. xv, 95 p. 12s. 6d. (U.K.) (paper covers).

Doctrines of personal identity in Locke, Hume and Reid, sceptical positions to which they give rise, and their resolution.

PLATO. Phaedo, translated with introduction by R. Hackforth. New York, Liberal Arts Press, 1960. x, 200 p. $1.25 (paper covers).

QUINE, Willard Van Orman. Word and object. Cambridge, Mass., Technology Press of the Massachusetts Institute of Technology and John Wiley & Sons, 1960. xv, 294 p. $5.50.

Discusses "the semantics of objective reference" from the viewpoint of a dispositional theory of meaning.

RAPHAEL, D. D. The paradox of tragedy: The Mahlon Powell Lectures [University of Indiana] 1959. London, Allen & Unwin, 1960. 112 p. 16s. (U.K.).

Discusses the essential nature of tragedy (a conflict between human heroism and impersonal necessity), its connection with religion (the outlook implied is incompatible with Christianity) and with philosophy (Plato was influenced by Greek drama, and, conversely, Sartre or Arthur Miller present themes that are philosophical in at least the broader sense of the word).

ROUSSEAU, Jean-Jacques. Politics and the arts; letter to M. D'Alembert on the theatre. Translated, with introduction and notes, by Allan Bloom. Glencoe, Ill., Free Press, 1960. xxxviii, 153 p. $4.
A defence of censorship.

SARTRE, Jean-Paul. To freedom condemned; a guide to his philosophy by Justus Streller, translated and with an introduction by Wade Baskin. New York, Philosophical Library, 1960. 163 p. $3.
Short extracts from Sartre, arranged under such headings as "Grace", "Facticity", "The Hole". (The hole is something which longs to be filled.)

SCHNEIDER, Herbert W. Morals for mankind: the Paul Anthony Brick Lectures, 1960. Columbia, Mass., University of Missouri Press, 1960. xiii, 82 p. $2.50.
The three lectures deal respectively with conscience, religion and ethics, and social obligations. Popular rather than philosophical.

SHARMA, B. N. K. A history of the Dvaita school of Vedānta and its literature; volume 1. Bombay, Booksellers' Publishing Co., 1960. xviii, 372 p. Rs. 17.50.

SOLMSEN, Friedrich. Aristotle's system of the physical world; a comparison with his predecessors. (Cornell Studies in Classical Philology, vol. xxxiii.) Ithaca, N.Y. Cornell University Press, 1960. xiv, 468 p. 77s. 6d. (Australian).
The influence of the pre-Socratics, and of Plato, on Aristotle.

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NOTES AND NEWS

A.A.P. ANNUAL CONGRESS, 1961

An Annual Congress for 1961 will be held in Canberra from August 21st to August 25th. Accommodation will be available.

The proceedings will open with the Presidential Address on the evening of August 21st. The Annual General Meeting will be held at 5 p.m. on Wednesday, August 23rd, and the dinner will be on that evening.

Further information may be had on application to the Hon. Secretary of the Congress, Dr. Robert Brown, Department of Social Philosophy, Institute of Advanced Studies, Australian National University, Canberra, A.C.T.



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DISSOLVING THE PROBLEM OF FREEWILL

By R. L. FRANKLIN

I

Many philosophers in the last two decades have argued that the problem of freewill and determinism is a pseudo-problem, to be dissolved by proper attention to the conceptual confusions involved. One set of considerations in particular has achieved very wide acceptance, and has been defended by many who differ considerably on other ethical problems. In so far as I here discuss particular philosophers I shall confine myself for convenience to Professor Nowell-Smith.[1]

It is agreed that a man is morally responsible only when, in some sense, he could have done otherwise. The central point of the view I am discussing is to maintain that there is no incompatibility between this responsibility and the possibility that the act was caused. The argument starts from the distinction between prescriptive laws (e.g. of the State) and descriptive ones (e.g. of the sciences). To be subject to the former is to be compelled, while the latter merely state what in fact always happens. So if descriptive laws of human conduct are framed, our actions in conformity with them are still "free" in the sense of "uncompelled". What then are we to make of the remark that in a free choice a man "could have done otherwise"? The analysis offered is that it means he "would have done otherwise if he had chosen". This does not require us to suppose that he could have *chosen* otherwise.

Now only two questions remain. First how do we tell in more difficult cases, such as kleptomania, that a person would not have been in a position to do otherwise if he had chosen? Secondly, why do we select this as the criterion for moral responsibility? The answer given to both these questions depends on the same point. Praising and blaming are social pressures to

[1] See "Freewill and Moral Responsibility", *Mind* (LVII) (1948), p. 45, and "Ethics" (Penguin) (1954).

induce people to behave in socially acceptable ways. To say that a person would not, for example, have mastered a craving, if he had chosen to try, is to say he would have not been influenced in his choice by praise or blame; an empirical statement of which we can often be pretty sure. And to say that if he is not capable of being thus influenced he is not morally responsible, is to say that when praise and blame are pointless they are pointless.

Thus "X was morally responsible for doing Y" means "X's act was capable of being affected by praise or blame". This is compatible with its being completely determined by descriptive causal laws, and incidentally presupposes at least a large measure of determinism if we are to be able to assess the likely efficacy of praise or blame.

I shall be referring to this theory so often that I need a name for it. As it claims to dissolve the traditional conflict between Libertarians and Determinists, I hope I may call it Dissolutionism.

II

Why do we exculpate such people as kleptomaniacs from moral blame? The Dissolutionist answer is that we do so because it is pointless. To this Nowell-Smith would certainly be prepared to add that blame in such cases would be morally wrong, a sort of mental cruelty. For there is a general moral principle that we should not inflict pain or unpleasantness on another without justification.[3] And if it is pointless to blame, the only justification for inflicting this unpleasantness is gone.

Both of these points are no doubt true, but they do not, it seems to me, give the right answer. The reason for refraining from blame in such cases is that we do not think the person *deserves* it. For if I receive blame which I do not deserve, I am always morally entitled to disregard it; and if I did deserve it, I never am. Desert is therefore both a necessary and a sufficient condition of being properly blamed.

The notion of desert will take much analysing, but it can be shown at once, I think, that this cannot be done in terms of amenability to praise and blame. Suppose I have done some action which would be blameworthy if I could have done otherwise, and a Dissolutionist criticises me for it. If I ask him to justify his blame, he must reply: "Well, your actions of this sort are, I believe, liable to be influenced by it." This is inadequate for two reasons. First, it does not mean the same as saying that I deserved it, or, for that matter, that I could have done otherwise. Suppose I had been hypnotised into the act, and also

[3] Cp. "Ethics", p. 299.

instructed under hypnotism that if I were blamed I would not do it again. Here the blame would be unusually efficacious but still not deserved. Secondly, once I know that his justification is simply that blame can in fact influence me, why should I continue to be influenced? Why should I not firmly resolve to ignore it in future? How, in fact, does this show me that I deserved it, and therefore should not disregard it? So, if I accepted his account of why his blame is justified, the blame would cease to have the efficacy which is supposed to be its justification.

I do not deny that my signs of amenability to blame may be the key factor in bringing anyone to believe that I could have done otherwise and so deserved it. But even if it were the only one (and I do not think it is), Dissolutionists would still have committed the fallacy of confusing the meaning of a proposition with the evidence in favour of its truth.

It might help to bring out my point if we contrast moral blame with the sort of case where we do seem to adopt a Dissolutionist criterion of amenability. When, say, a small child is subject to temper-tantrums, his parents may ask: "How should we deal with this? Is it a case where firm discipline will help him to get over it? Or should we, believing that tantrums spring from emotional insecurity, on such occasions soothe and reassure him?" The approach here is therapeutic. The parents are asking not whether he can help having the tantrums, but how he can best be helped to get over them. This is certainly proper. But to treat him in this way is, precisely, to treat him as a child; that is, as one who is not in this respect a fully moral agent.

III

The paradigm, if not perhaps the only, case of moral responsibility is where a man acts deliberately on some accepted principle or rule or policy. Nowell-Smith persuasively links this fact with his general position.[3] For, apart from insanity, in such cases praise and blame (and even rational argument) may conceivably weigh with the agent. But there are unfortunately men who act on the set and deliberate policy that moral criticism is to be sneered at or ignored when it conflicts with their chosen aims. These men are, naturally, as unamenable to moral praise or blame as any kleptomaniac.

Holding fast to amenability as the criterion of responsibility, Nowell-Smith is prepared to treat such people as not responsible. "If we really thought a man was so bad as to be irremediable", he says, "we should, I think, cease to blame him, though we might

[3] "Ethics", Ch. 20.

impose restraints on him as we would on a mad dog".[4] This is simply false. We know only too well that power-drunk tyrants and cold-blooded gangsters are not amenable to moral pressures in those areas of their lives which we consider most vicious. Yet surely no one, including Nowell-Smith, wishes to exculpate them from blame on that ground? I do not mean that judgments are always easy in this field. The very depth of a man's callousness may sometimes make us—at least at a safe lapse of distance or time—wonder whether he could truly have been sane and responsible for his actions. But we are not wondering whether he could have been influenced by praise or blame. Rather it is the evident fact that he could not which raises the question about which we do wonder. We do not exculpate the incorrigible when we believe them to be incorrigibly wicked. The reason is the same as that brought out in Section II. They are blamed because they deserve it.

IV

I must now do what I can to analyse this concept of desert. Philosophers are accustomed to discuss it not so much in relation to the nature of moral responsibility, as in the different, though related, context of the justification of punishment. To talk of desert reminds one of retributive, as opposed to deterrent or reformative theories.

For the Dissolutionist, both blame and punishment are social pressures to minimise undesired conduct. This is too simple, as Professor Campbell in his critique of Dissolutionism has pointed out. "It is quite possible", he says, "by punishing the dog who absconds with the succulent chops designed for its master's luncheon, favourably to influence its motive in respect of its future behaviour in the like circumstances."[5] On Dissolutionist principles we should therefore hold the dog morally responsible, yet of course we do not. Campbell's own view is that punishment contains "an element of requital for moral transgression";[6] i.e. that punishment *presupposes* that the agent was morally responsible. Unfortunately, this is incompatible with his own starting point, that we *do* punish the dog who is *not* morally responsible. It therefore seems that the relation is more complicated than he too has seen.

It seems to me that the notion of desert does imply the belief that there should be in some sense a proportionate retribution. But the crucial question is, what does a man deserve blame

[4] "Ethics", p. 306.
[5] *Mind* (1951), p. 447.
[6] *Ibid.*, p. 448.

or punishment for? The accepted answer is that his desert depends on his intention, on what he aimed at doing. "He didn't mean to do it" is always a good moral defence. The agent may still be open to another lesser charge, such as "Well, he shouldn't have been so careless". But all such charges are themselves logically linked in various ways to what he aimed or should have aimed at doing.

This sort of account is frequently offered as a description of how our moral thinking actually proceeds. I now wish to point out that, as such, it is incorrect. On the contrary, in apportioning blame or punishment, men persistently take into consideration not only the agent's intentions but the consequences of the action. Two examples are enough. A child carelessly drops a cup. To how many parents would it make no difference in apportioning the blame whether it was a valuable antique or could be cheaply replaced from the nearest chain-store? Again, the law punishes attempted murder less seriously than actual murder. There are no pragmatic reasons, such as deterrence or special difficulties of proof, to justify this distinction. Yet to how many people in the community does it seem obviously unjust? Indeed if it has any better explanation than legal conservatism, it is probably just that, since the ordinary man does take consequences into account, juries might refuse to convict of attempted murder if it carried the highest penalty.

But, it will be objected, we clearly *ought* not to let the value of the cup, the success of the crime, weigh with us in apportioning blame or punishment. I think I would agree. But I want to bring out what a tremendous piece of moral legislation this is. It no more accurately describes the ordinary man's *beliefs* than a moral principle describes his *conduct*. This should puzzle philosophers like Nowell-Smith, I think, who claim merely to be analysing the moral concepts people do in fact have. On this issue philosophers seem to go in for the unfashionable procedure of telling the plain man what he ought to believe; and if so, we had better be pretty clear both why they do so and why he does not believe it.

The explanation seems to lie in the *development* of our notion of what a man deserves. It arises, I suspect, out of the need to put limits on the spontaneous primitive anger of a man who has been harmed, whether intentionally or not. The natural first limitation would be the crude equality of "an eye for an eye and a tooth for a tooth", where intention is still ignored. The notion then enters on an enormous process of refinement, leading to subtler discrimination between externally similar cases, and, as moral concepts develop, to ever-increasing emphasis on inten-

tion. The notion may therefore enter into our moral thinking with varying degrees of crudity or sophistication, and the result depends on the subtlety with which it is applied. This may illuminate our puzzles.

The ordinary man and the law to a large extent agree that if a man could not have done otherwise, all retribution is irrelevant. If, however, retribution is in order, both common morality and common law often proportion it fairly crudely to the extent of the damage done. Those who maintain that we should look only to the intention in blame and punishment are recommending, for reasons we have not so far discovered,[7] that since the real evil is the intention, the retribution should be proportionate solely to that. The gap between the philosophers' or moralists' standards and those of the ordinary man can then be seen as a clash between a sophisticated and a crude application of the same principle, and thus becomes a fairly typical example of development in moral thought. We might also throw light on Campbell's inconsistency about the dog. It is quite comprehensible that the tendency of primitive law and morality to punish irrespective of intention might survive as one strand in the meaning of "punishment". We would then talk of punishing when we mean no more than the infliction of a proportionate penalty for the breach of a rule. And this, surely, is all we *can* mean when we talk, as we quite naturally do, of punishing a dog.[8] On the other hand, when we use "punishment" in a morally more developed context, we so far agree that intention is the real evil that we treat an evil intent as a prerequisite, even if we only too often consider other factors in apportioning the amount. In these cases therefore it is true that punishment contains "an element of requital for moral transgression".

My contribution to the analysis of this concept of desert may be taken then to be these points: that it lies at the base of our notions of moral responsibility and punishment; that it arises from the need to restrict the reactions of a harmed man to the exaction of an equal retribution, but assumes that such retribution is due; that it becomes increasingly attached to intention; but that there remains an immense variation in its application which may account both for some puzzling features of our moral attitudes and for some inadequacies in the views of philosophers. As for what I have not done: not only have I left many intricacies of the notion unmentioned, but I have not shown that it *ought* to enter into our thinking as it does. A man could for example argue

[7] See end of Section VI.

[8] Though perhaps some people half-believe that the dog "should have known better".

that efficacy of praise and blame ought to be the only criterion, and that retribution is an outmoded superstition. Or he could concede the notion of desert in the realm of moral responsibility while maintaining that legal punishment, at least, can only be justified on such grounds as reformation or deterrence. My point is simply that any plausible challenge to the notion of desert must be a claim that our morality should *outgrow* the notion. In so far as Dissolutionists usually claim to be analysing the concepts we actually have, their account of punishment and blame is inadequate because it ignores the element of retribution.

V.

The Dissolutionism I have discussed is in effect a crudely Utilitarian theory of praise and blame which cannot do justice to the notion of what a man deserves. Could it be modified to cope with this? Yes, I think, by treating it in a way analogous to that in which Nowell-Smith himself has modified Utilitarianism in two other respects. Granted, he argues, that the latter gives a bad answer to the question "Why should I do this?", it still answers the different question "Why do we have the moral rules we do?". I do this because there is a moral rule to this effect; but society has this moral rule because it is believed to make for the happiness of all.[9] An even more helpful analogy is his revision of his views on punishment. In 1948 he accepted a "Utilitarian theory of punishment . . . Punishments are distributed not because certain actions directly 'merit' them, but because some useful purpose is believed to be served by inflicting them".[10] But by 1954 he had distinguished between the judge's question and the legislator's question. The judge should decide whether to punish by asking whether a law has been broken. The legislator should decide what acts shall be punishable by, roughly, Utilitarian criteria.[11] We, as individuals, occupy sometimes one role and sometimes the other in our moral thinking.

Since moral responsibility is analytically tied to praise and blame, and since the latter are taken to be special cases of reward and punishment, it seems to me quite manifest that a crudely Utilitarian theory of moral responsibility cannot survive the abandonment of such a theory of punishment. But an analogous modification could be made. We exculpate people who could not have done otherwise, it might be said because they do not deserve it (cp. Sec. II above). We condemn the incorrigibly callous, as

[9] "Ethics", pp. 232-6.
[10] *Mind* (1948) at p. 59
[11] "Ethics", pp. 270-3.

Nowell-Smith ought to have admitted, because they do deserve it (cp. Sec. III inf.). But we have achieved, and we preserve and teach, this notion of desert for reasons of maximum social utility.

Of course the social utility of this principle must be defended, but this does not seem too hard. Let us take first the case of condemning the incorrigible. There are obvious social advantages in not exculpating them; the difficulty of knowing that they really were incorrigible, the bad effect on others who were tempted to stifle the last remnants of their consciences, and so on. The utility of exculpating those who could not have done otherwise needs a little more elucidation. It is simply an application of the principle that our praise and blame should be directed towards a man's intentions; for if he could not have done otherwise he had no deliberate evil intent. Now only a man's intentions are capable of being affected by thought of praise or blame. Hence though it is socially useful to try to minimise incompetence and bad consequences as we do, by blaming people for careless disregard of probable results, for taking on tasks for which they were unfitted, or for failure to develop their talents, the general social utility of connecting praise and blame to intention seems obvious.

This theory then could talk with the rest of us of what a man deserves, while still grounding the whole story at one remove on social utility. And since it is already justified in this way, what need is there to make our praise and blame dependent on mysterious intuitions of contra-causal freedom? Thus it still dissolves the problem as surely as Nowell-Smith's simpler view. I fear however that there are yet other reasons why I cannot accept it.

VI

It is certainly true that only intentions can be influenced by praise or blame. But this modified Dissolutionism must make a different claim; namely that a man's intentions are most effectively influenced when we confine our praise and blame solely to them. This is almost certainly false; and, even more important, it is undoubtedly believed to be false.

The truth is that in situations where our society holds that results are more important than justice to the individual, it regularly holds the individual responsible both for incompetence and for bad consequences. A clear case is that of command in wartime. A large part of what "the responsibility of command" means is that the commander is *prima facie* responsible for results, and indeed for the results of what other people do. A defeat is not followed by any nice investigation into whether the general could have tried harder, or whether he had used his time

at Staff Training College to the best advantage. This is not because such investigations are too difficult, but because they are irrelevant. The question is not about this man's efforts but about something else; perhaps whether it was humanly possible to avoid defeat. If it was, he is censured for not avoiding it, and very likely relieved of command *pour encourager les autres*. This is part, though of course only part, of the whole complex of considerations which normally produces in generals such a wholehearted dedication to the defeat of the enemy. Certainly to set the standard too high, to ignore all excuses in all circumstances, is probably self-defeating in a number of ways. But there is excellent evidence that we obtain the best results, because we drive people to exert themselves to the utmost, if we hold them responsible for rather more than it was actually in their power to control or achieve.

To censure commanders for failure to achieve results or for incompetence is presumably not to censure them morally.[12] But why is it not? In one sense the answer is obvious; because their intentions were not at fault. But my question is: why is moral censure thus linked to intention (and so to cases where the agent could have done otherwise), when other forms of censure often are not? Surely Dissolutionism and social utility can give no answer. Such censure of commanders is just the sort of social pressure that ordinary Dissolutionism says moral blame is; and far from being pointless it is engaged in because it is believed to be effective. The modified Dissolutionism which I sketched may indeed distinguish between this censure and moral blame by pointing out that it is not connected to intention, and thus not morally deserved, but it can now no longer explain the principle of desert itself by reference to social utility.

I have reached the place where I should say something constructive to replace the view I have criticised. Why *do* we say men are morally responsible only for what they intended to do? No doubt this can be taken as a tautology, an elucidation of the meaning of *moral* censure. But it is also normally part of the meaning of moral judgments that they are the most fundamental sort of appraisal of our fellow men that we can make. These cannot both be tautologies at once. Between them, they assert the vital synthetic proposition that, however we may need to treat our generals in wartime, our most fundamental appraisals of our fellow men should be based on our assessment of their intentions.

[12] Whether a man who takes command, *knowing all that this implies*, has a right to complain of injustice in such cases, is a subtle moral question on which I make no comment.

If this cannot be justified by our conception of maximum social utility, how can it be?

The proposition seems to me in fact to involve a protest against setting social utility, the persuading of people to behave in socially acceptable ways, as the highest value. It seems to follow from the belief that the ultimately valuable things are human beings and their personalities, and that society therefore exists for the sake of its members and not *vice versa*. And how can this belief in turn be justified? I am not prepared now to try to answer. We do not begin by rationally weighing the arguments for our basic presuppositions, but only justify them if we can when challenged. Nor do we suspend them while we consider our justification. In the last resort, in the very long run, we may have to stand or fall by the reasons which could be produced. Meanwhile, we are simply aware that there are some judgments in making which, as it were, we express our deepest attitudes towards our fellows. One such, for most of us, is that a man deserves the most fundamental sort of approval and disapproval only for his intentions, and so only when he could have done otherwise. I do not think Dissolutionism or social utility can give adequate reasons for this, but I still believe it to be true.

VII

I turn to the most debated question in this sphere: does "X could have done otherwise" mean merely "X would have done otherwise if he had chosen", or does it also imply, as Libertarians insist, that he could have *chosen* otherwise?

To base praise and blame on desert rather than on efficacy leads naturally to a Libertarian view. A man's desert depends on his intentions, because only these are, as we believe, normally under his control. But would such control be compatible with complete causal determinism? Even when we strip away all possible confusion between prescriptive and descriptive laws, a causal generalisation entails in principle: given situation X it is a fact that always Y and only Y. If such laws could be framed for human conduct, no matter what concepts were used in their formulation, then given X, the sum total of relevant factors in your situation, it is a fact that you can make only one decision Y. And since X is itself determined, it follows that given your heredity and the total stimuli you receive, it has never been possible since your birth, or indeed since the universe began, that you should choose otherwise than you shall choose in ten minutes' time. If we believe this we could still acknowledge that approval and disapproval are factors which affect men's choices.

But does it not seem to evacuate of meaning the notion of a man's action being "under his control"?

There are two direct Dissolutionist counters to this, one implausible and the other impressive. The implausible one is, in effect, to deny that a belief that the agent could control the action enters into our notion of responsibility The Libertarian, Nowell-Smith has said, supposes "that when we say 'A could have acted otherwise' we mean that A, being what he was and being placed in the circumstances in which he was placed, could have done something other than what he did".[13] Having thus put into his opponent's mouth a lucid account of what we obviously do mean, he goes on in his next sentence to add, "But in fact we never do mean this". He proceeds to assert that we only mean that he would have decided differently in different circumstances or if his character had been different.[14] But, as Campbell asks: "What conceivable significance could it have for our attitude to A's responsibility to know that someone with a *different* character (or *A* with a different character, if that collocation of words has any meaning) or A in a different set of circumstances from those in which A as we are in fact concerned with him was in fact placed, 'could have acted otherwise'? "[15]

The other, and impressive, counter-argument is that something must have gone wrong with the Libertarian analysis of "control". For the whole notion only gets going by distinguishing between, say, my desire for a cigarette, which I can control, and the addict's craving for cocaine, which he cannot. How could any argument validly show that we ought to give up this distinction? I cannot treat this at the length it deserves, but I think the Libertarian may properly reply in this way. It makes sense to suggest that an action which we thought was under the agent's control really was not. We came to believe this in the case of kleptomania, and even a man's beliefs about his own actions may be altered by what a psychiatrist reveals to him of himself. If this is so, we cannot dismiss *a priori* the possibility that analysis of what we mean by "action under our control" might show that the notion rests on a pre-supposition such as causal indeterminism. And the argument I have given seems to me to show that it does.

But there are forms of Libertarianism from which I must dissociate myself. In particular, there seem to me insuperable difficulties in Campbell's view that we are free and responsible only in those choices where we are conscious of a clash between moral duty and strongest desire. Apart from some other cogent

[13] *Mind* (1948), p. 49.
[14] *Mind* (1948), p. 50.
[15] *Mind* (1951), p. 453.

criticisms by Nowell-Smith, very often a man is responsible for a choice where only one course of action is a live option for him. We do not say of a bank-clerk: "He deserves no praise for not stealing the money. He is such an honest man he couldn't help it". Yet if *sometimes* a man is clearly responsible though his act flows predictably from his character, why should the situation be different if, as Dissolutionism suggests, it *always* does?

It is this step from sometimes to always which I want to deny. If a man so accepts any value as his own, that his character is firmly set in accordance with it, I see no reason why we should not, as it were, take him at his principle, and hold him responsible for the act that flows from it. Not to do so, indeed, is ultimately to treat him as less than a responsible human being. But *he,* we believe, must have accepted these values, and formed his character; not uninfluenced by heredity and environment, of course, but with sufficient control over the result to be responsible for it. And he could not have done this, he would not be a responsible agent, unless some of his choices had been between genuinely open possibilities. This is the logical presupposition, as it seems to me, of treating men as beings who can control their acts and destinies, which is itself our presupposition in treating them as moral beings who can deserve praise and blame.

This raises for me a new difficulty which I cannot discuss in detail now. It is normally and naturally assumed that whatever "could have done otherwise" means, it always means the same. I do not think I can say this. In so far as I can accept the standard formulation of the problem, I would have to say that the phrase always involves at least the Dissolutionist "would have done otherwise if he had chosen"; that this is sometimes sufficient; but that if it never involved the Libertarian "could have chosen otherwise" we would never be responsible.

Of all the arguments remaining against Libertarianism, I can discuss only one, which throws into relief some of my points. If you suppose a contra-causal freedom, I may be told, you are making an assertion which could in principle be open to empirical disproof. Surely you do not mean this? As Stebbing argued in relation to Libertarians' appeals to Heisenberg's Uncertainty Principle: if strict causality were re-established in physics, would this shake their confidence? She answers: "Assuredly not; how, then, can the results of the work of the physicists have any bearing upon this intuition?"[16]

This dilemma seems to me a perfectly real one. Hence, I must just accept that horn which Stebbing assumed was unthink-

[16] *Philosophy and the Physicists,* Methuen (1937), p. 287

able, and concede that our basic notions of moral responsibility logically could be revolutionised by scientific discovery.

To be more specific, no conceivable discoveries in psychology need worry the Libertarian, I think, though I have no space to elaborate my reasons; but the case seems different in neurophysiology. It does not seem theoretically impossible that we might establish, first, that between mental events and brain events there is a precise correlation, and secondly that brain events are completely subject to determinate laws of physics. In that case we would have established, I believe, that we are not free in the way we take ourselves to be. That is, the implications of such discoveries would be incompatible with the implications of our moral appraisals.

I have three comments to mitigate the paradoxicality of this view. The first is that to say we cannot disprove a possibility is not to suggest that it is the case. Scientists may very properly take determinism, if they do, as one of their working assumptions, but it is notorious how these can change. To claim that neurophysiology actually lays a basis for determinism would be to base a piece of bad metaphysics on an extrapolation of currently useful assumptions.

Secondly, the immediate relevance to us of this possibility can be no stronger than the actual apparent likelihood of its being established. We have all the evidence which at present we could have that men can control their decisions, and a man may not base an excuse on the bare logical possibility that one day a scientist will find it for him.

Finally, if this possibility should ever eventuate, what would be the result? Moral rules, and obedience to them, would still be no less necessary for society, but how would men feel towards them? They would take either of two attitudes, I would guess. Some might say: "I deserve neither praise nor blame, for desert is now a meaningless notion." In that case blame might cease to weigh with them at all. But there is another possibility. Often even now a man may so identify himself with some group that he feels a satisfaction with its achievements, or a shame at its shortcomings, which is introspectively no different from his feelings about his own. It is not inconceivable that a man might similarly accept responsibility for his own actions even if he believed himself never to have open possibilities of choice. Such a morality founded on acceptance of the inevitable rather than on freedom would not be our morality, but it would not be impossible or absurd. The implications of the change would be far-reaching. A man, for example, may accept responsibility for what he could not control, but could we impute it to him if he repudiated it? On

the details of such a new conception it would be unprofitable to speculate. The only thing which seems to me beyond question is that it would produce a challenge to our thought, compared to which the Copernican revolution would seem a mild and transitory disturbance.

The last paragraph is not intended as an essay in science fiction. To contemplate the possibility of determinism seriously seems to me to bring out two points of value. The first is that this really is inconsistent with what we do take for granted when we hold people morally responsible. The second is that only much and not all of what most people value is at stake. Some people have doubted Libertarianism because they have felt that surely not all our values could be at the mercy of conceivable future scientific discovery. I believe they are right, but that this does not show that our present view of man does not presuppose contra-causal freedom.

It would be nice to believe that our basic assumptions on moral responsibility are not merely well-grounded but fixed beyond challenge. If this cannot be done, it would at least be nice to know what the future will bring forth. But if I am right, there is a sense in which in the last resort we will have to wait and see.

University of Western Australia.

DEFINITION BY INTERNAL RELATION[1]

By JUDITH JARVIS

1. At the beginning of his *Treatise of Human Nature,* Hume sets himself the task of proving the following proposition or maxim: "That all our simple ideas in their first appearance, are derived from simple impressions, which are correspondent to them, and which they exactly represent." Freely translated into contemporary jargon, this maxim may be read as follows: The meanings of the names of simple properties (properties such as red, blue, bitter, sweet, and the like) can only be learned by ostensive definition, which is the process of pointing out samples and labelling them—e.g., I ostensively define the word "red" when I point out red things, saying of each of them "That's red", and point out things which are not red, saying of them "That's not red". As Locke had said: if the simple ideas "are not received this way, all the words in the world, made use of to explain or define any of their names, will never be able to produce in us the idea it stands for".

To be more precise, and to forestall certain irrelevant objections, Hume's maxim should really run somewhat as follows: The meanings of the names of simple properties can only be acquired by one who, at some time, sees (hears, tastes, whatever is relevant) samples of the property, and hears or sees the name associated with them. But I shall not bother spelling all this out carefully; it can be abbreviated in the words: "the meanings of the names of simple properties can only be learned by ostensive definition".

Nevertheless, Hume suggests that there is an exception to this maxim. I shall not quote the passage from the *Treatise*; the point may be put in the following way. Imagine that we take a strip of paper on which is painted a continuous band of shades of blue from very dark to very light, and that we cut out a small bit of it; imagine further that we paste the two remaining halves on another sheet of paper, leaving a small space between them. Let us call what we have now constructed a "broken band". Now imagine a man who has never seen anything of the shade which has been removed in constructing this particular broken band. We show him the broken band, and tell him that the name of the missing shade is (let us say) "boole". We then

[1] I am grateful to Professor Sidney Morgenbesser for his helpful criticisms of earlier drafts of the following paper; he is of course not responsible for any errors which may remain.

show him a handful of colour-cards, all of different shades, one of which is in fact boole in colour, and we ask him which. Now it may be that he will not be able to tell; but unfortunately for Hume's maxim, it may be that he will be able to pick out the right one. I say "unfortunately" because, according to the maxim, a man should *not* have been able to acquire the use of a name of a simple property (colours are surely simple properties) unless he has been shown things which possess this property—and this man has not.

Hume says of the fact that a man may be able to do this—recognize something as boole in colour, without ever having seen anything of that shade before—that while it is indeed an exception to the maxim, nevertheless "the instance is so particular and singular, that it is scarce worth our observing, and does not merit that, for it alone, we should alter our . . . maxim". But exceptions are unfortunate in philosophy; something is wrong if they can't be accounted for in some way or other.

The first point to be made, I think, is this: the instance is nowhere near being so "particular and singular" as Hume suggests. Let us call the possibility described above "Case A". Here is Case B: we take a strip of paper on which is painted a continuous band of colours from red at one end to violet at the other, and passing in between through orange, yellow, green, blue, and indigo—the spectrum, in fact. We cut out just the bit of it which is clearly green, leaving in the yellow-greens and the greenish-blues, and construct a broken band out of the two remaining bits. We tell a man who has never seen anything of the missing colour that the name of the missing colour is "clear green"—and it *may* be that he can then recognize some clear green thing which we then show him *as* clear green. That is, it is not only when shades of one given colour are involved that the procedure may work.

And if we consider again Cases A and B, I think we can see that it may be that the man to whom we show the broken band will not only be able to pick out the right colour-card which we offer him on that one occasion (he says, picking the right one out, "This is the one that's boole" [Case A], or "This is the one that's clear green" [Case B]), it may be that he can *continue* successfully picking out the boole or the clear green samples, can in fact use the word as properly and as well as one who had been taught its use by a process of ostensive definition. This man has apparently acquired the use of a name of a simple property—for boole and clear green are colours—*without* ever having seen samples of those colours.

And in fact we needn't even restrict ourselves to the names of colours. But before proceeding to other cases in which learning

takes place in the same manner, I should like to mention a *second* maxim which we are familiar with in philosophy. I do not call it "Hume's second maxim", though it is to be found in Hume; I shall call it instead, "the general maxim". It may be put as follows: learning the meaning of a word is *either* learning it ostensively (or more carefully, learning it in a manner which involves during the course of the learning an acquaintance with samples and non-samples and the names associated with them) *or* learning it by discursive definition (a process of setting out equivalences between words or phrases or whole sentences) into a conjunction or disjunction of expressions whose meanings either have been or can then be learned ostensively.

I now wish to proceed to Cases C and D, which do not perhaps violate *Hume's* maxim, but which nonetheless violate this *general* maxim. In Case C, we construct a set of what I shall call "blocks": objects of different materials, shapes, and sizes, which we have cut in half, each half hollowed out in such a way that when the two halves are held together, the volume enclosed is a sphere. Now here is a man who has never seen anything spherical in shape; we tell him "A sphere would fit smoothly into *here* [pointing to a block]—and another would fit smoothly into *here* [pointing to another block of a different size]—and another into *here*, . . ." And it *may* be that he can then recognize as spherical in shape, as a sphere, some sphere which we then show him—even some sphere which would *not* fit smoothly into any of the sample blocks we have just shown him.

Of course there may be many things about the use of the word "sphere" which this man, having learned the use of the word in this way, may not know—I have in mind the geometrical properties of spheres. Nevertheless this still seems to be true: this man may be able to pick the spheres out from any collection of objects we show him, without either having seen any spheres or having been given one of the customary discursive definitions of the word "sphere"—as, e.g., "a sphere is the volume enclosed by the surface which is the locus of all points equidistant from a given point".

Similarly, Case D: we may show a man various pictures of giraffes (or better, perhaps, animated cartoons of giraffes), and tell him that *this* is a picture of a giraffe, and that is, and that is; and it *may* be that, from this, he can recognize as a giraffe the giraffe we then find at the zoo the next day. It may be that, without ever having seen a giraffe, and without ever having been given a discursive definition of the word "giraffe", he can nevertheless use the word as properly and as well as one who acquired it in some other way.

2. A complaint which may arise here is the following: "*Of course* you can construct cases of this sort, where a man may be understood to have acquired the use of a word 'X' without ever having seen any sample X's and without having been given a discursive definition of the word 'X'. The whole thing's a mistake from the beginning. The question what must be present or what needn't be present in the process of learning the use of a word is an empirical question—in fact a question in empirical psychology. Now it may perhaps be granted that a philosopher is entitled to make empirical claims—but certainly not when he has performed none of the experiments relevant to establishing those claims. I submit, then, that Hume is not entitled to make a claim of the sort you have called 'Hume's maxim', nor are the philosophers who are so fond of repeating what you have called 'the general maxim' any more entitled to make that claim than Hume is to make his own. For, to my knowledge at least, none of them has ever supported his claim with any reports of experiments on the learning processes of children."

I think that this suggestion, that the two maxims we have mentioned are *empirical,* not *a priori,* is both true and false—but that its truth is not of a sort which need send us to the psychologists for reports on experiments in learning.

To begin with, consider the following description, which we can call Case Q. We are in the operating room as a child is born; as soon as it begins to breathe, we hold a ripe tomato before its face, and ask "What colour is this?" "Red", the child replies. We are stunned, and try it out on other objects; it gives the answer "Red" only where the object is red, and of every other object says "Not red"—it always gets the answer right. In fact, it then proceeds to use the word as properly and as well as any of us.

Or again, Case R: the baby can't recognize red things as red at first—it gives no answer at all when we ask for the colour—but then we gently wave a bit of birch bark over its head, and *now* it can, now it is as in Case Q. Imagine that this only works with bark from a birch tree, and not with bark from any other kind of tree—i.e., we have tried waving bits of bark from all other kinds of tree over its head, but the thing only works at last with birch bark.

Are these descriptions of possible situations? *Could* these things happen? "Well", you say, "I can easily imagine them. I can form the picture in my mind. I can imagine seeing them: there is the baby, there *you* are with the ripe tomato, now you hold it up before the baby's face, and so on and so on. So it follows that though it's most unlikely, most improbable, that any

such things *should* ever actually happen, nevertheless they *could*. The descriptions do not describe the logically impossible."

I want to agree that these situations can be imagined or pictured. But I want to insist that the force of the expression "logically possible" here is just that and no more: they can be imagined or pictured. That is, I want to bring out the temptation to say: I don't care whether or not they can be imagined or pictured—they *couldn't* in fact happen! That is, the temptation to say that these are *not* mere ordinary, run-of-the-mill improbabilities, but something far worse.

To begin with, we may notice that if anyone told us that these things had in fact happened, we should refuse to believe him—we should accuse him of dishonesty or naiveté, depending on which aspect of his character we had least confidence in. But our reaction would be the same if a man told us that he had made observations which clearly indicated that the back of the moon was really covered in green cheese—and this, as I should want to say, is a mere ordinary, run-of-the-mill improbability. It isn't just the fact, then, that we should try to find another explanation, an explanation which made clear that the events did not really happen as described, which distinguishes the situations described in Cases Q and R from mere ordinary, run-of-the-mill improbabilities and which then tempts us to say that they couldn't *really* happen.

What tempts us to say this is the following. If such a thing were to be seen as we have imagined or pictured to happen, no explanation which anyone could conceivably produce would adequately explain *how* it had happened.

Consider Case R—the baby can't recognize red things as red at first—but then we gently wave a bit of birch bark over its head, and now it can, now it has the use of the word "red". But *how* could waving a bit of birch bark over a child's head bring it about that he has the use of, knows the meaning of, the word "red"? Suppose the scientist who has been investigating the case tells us something like this: "The birch bark secretes a certain efflorescence (electricity, cosmic rays, whatever you like), which then coagulates in the baby's cerebellum, thus bringing it about that he has the use of, knows the meaning of, the word 'red'." Some description, in other words, of the effects which the waving of the birch bark had on the baby's brain. But in fact *nothing* of this sort—no description, however elaborate, of the effects of the birch bark on the baby's brain—will adequately explain the effect that the baby then has the use of, knows the meaning of, the word "red".

It *may* be of course that we shall some day find some descrip-

tion of a set of changes in a man's brain which we are prepared to accept as an explanation of his now having a certain habit or disposition which he did not formerly have—for example, the disposition to utter the word "red" whenever a red object is held up before him in broad daylight when he has his eyes open, and "not red" whenever an object which is not red is held up before him in broad daylight when he has his eyes open. But having the use of the word "red" is not having a disposition in this sense at all. I take it that there are many of us who have the use of the word "red" but who do not find ourselves constantly uttering the words "red" and "not red" whenever we are in circumstances of the sort described, in the way in which one finds one's foot kicking whenever one's knee is tapped with a hammer.

The baby's behaviour is far more complicated than this; we hold a succession of objects before him (asking "What colour is this and this and . . . ?") and he says "red" of all the red ones and "not red" of all the not-red ones; we say "Fetch the red ball on that table and put it on top of that cabinet", and he does so; etc., etc.; and we are all along very careful to show no signs whatever of amazement or approval when he gets the answers right and obeys the commands and requests correctly. The thing keeps up so perfectly that we are at long last, despite our amazement, forced to conclude that he does indeed know the meaning of the word "red". Of course we could still be wrong; other explanations are still possible; but I imagine we keep this up for a very long time (we try him out on the more obvious sorts of tricks, too—red spectacles and red lights and . . .), and when he says, as he is bound to if he really does know English (it's a good sign that he does), "What kind of tiresome nonsense game is this after all?" we tell him to be patient, it's all part of an important scientific experiment; and after a long while, the suggestion "Still, other explanations are possible" begins to look as absurd for him as it would be for you or me.

Moreover, we have been, as I mentioned above, extremely careful not to show any signs of approval in the process of testing him. So we cannot say that he learned the meaning of the word "red" *during* the testing process; this baby apparently knew its meaning all along, right from the beginning, right from the moment we waved the bit of birch bark over its head.

How does his behaviour with the word "red" come to be so good? Imagine the scientist tells us: "By waving the bit of birch bark over the baby's head, we have altered the state of his brain in such a way that when light of a certain sort strikes his retinas, and sounds of a certain sort (e.g., the sounds 'What colour is that?') strike his eardrums, the effect is produced that the sound

'red' comes from him—also, that when sounds of a certain sort (e.g., the sounds 'Fetch one of the red ones from the kitchen table') strike his eardrums, the effect is produced that he goes into the kitchen, and, when there, when light of a certain sort strikes his retinas, his arm reaches out to pick up one of the red ones, which he then brings back with him—also, . . . Or, that, when his physical condition has altered in a certain way, the effect is produced that, not the sound 'red', but instead the sound 'What kind of tiresome nonsense game is this after all?' comes from him. Etc."

But what has happened now is this: the baby's behaviour with the word "red" was so good, was so like that of one who, as we should say, really does know the meaning of the word "red", that we could no longer say this was a mere disposition in the baby, that his utterance of the word "red" and his behaviour on all these occasions was a set of mere *reactions* to certain stimuli, but had to say instead that (on at least some of these occasions—it is not required that this be so for all of these occasions) it was a set of *responses* to certain stimuli (questions, commands, etc.). The explanation which the scientist now provides, however, makes clear how the baby's success *is* a set of mere reactions—and *not* a succession of responses. And this means we shall say: he doesn't know its meaning at all. If it can be shown that a man's behaviour with a certain word is accountable for as a machine's behaviour is accounted for (or as the behaviour of his foot, when his knee is tapped with a hammer, is accounted for), then we shall no longer suppose he does what he does with knowledge.

"Mightn't it be that the scientists will one day discover that it is like this with all of us—even those who, as we should say, really do know the meaning of the word 'red'—i.e., that our knowledge of it is a disposition, that our behaviour is a set of reactions to stimuli?"

When you ask "What colour is that?" I look and say "red"; if you then ask me why I said this, I may reply, among other things, "because you asked what colour it was!" A (weird) case may be imagined in which I am telling you with this what was the cause (one of the causes) of the word "red" coming from my mouth; but in the normal case I do not intend this; what I intend is, rather, that this was my reason (one of my reasons) for uttering the word "red". But if I did utter "p" for a reason, then what happened is not susceptible of a causal account. For to say that I did A for a reason is to say that I did A because I chose or decided (for that reason) to do so; and where shall my choice or decision enter into a causal account of my doing A? But unless it enters, my doing A has not been accounted for. (1)

"Mightn't it be an irrelevant accompaniment? I.e., we think we do this and that for this and that reason—but in fact we don't; everything we do comes about as an effect of some cause." An actual case of this would be: my arm is paralyzed, though I do not know this; you have attached wires to it, though I do not know this; you now ask me to raise my right arm, and just as I look to be about to begin to do this, you press the button and because of the current, my arm goes up—though I think *I* raised my arm as a response to what you said (your having asked me to do so, my having wanted to please you, . . .). But cases like this are, to say the least, exceedingly rare. In fact, necessarily so: to suggest that it is perhaps like this for all our actions is to suggest that all our decisions are, as it were, purely internal; that it is only by sheer fortunate accident (so far as anything *I* do is concerned) that those parts of my body which I decide to move do get moved, and at just the moment at which I decide to move them. But it is absurd to suppose this a merely contingent fact, to suppose it a merely contingent fact that the motions of my limbs don't always take place, say, five seconds before I decide to make them, my decisions, so to speak, always desperately trying to overtake my actions. (2) "Mightn't the choice or decision enter as a link in the causal chain?" What would this link be? The thought of, or "click of acceptance" of, that which served as the reason? But in what mode should this exert a causal influence? Moreover, I may very well do A for a reason without having thought of that which was my reason for doing A prior to or while doing A. I may only become aware of what my reason for doing A was when you later ask me what my reason for doing A was. To say that I did A for a reason (hence, chose to do it) is by no means to say that my doing of A was preceded by any conscious process of deliberation—though of course it may have been.

Obviously nothing in the preceding constitutes a denial of the fact that scientists may some day become far more successful at making predictions than they now are.

"What waving the bit of birch bark over the baby's head does is to alter his brain in such a way that when light of a certain sort strikes his retinas, the effect is produced that the thing in front of him looks red; this, and (perhaps) the asking of the question 'What colour is that?' he takes (or does not take) to be reasons for saying 'red'." A thing may be said to look red to a man (say a Frenchman) who does not know the meaning of the word "red"—but a thing's looking red to such a man is *for him* no reason at all for uttering the (English) word "red" in response to the (English) question "What colour is that?"

asked of it. A thing's looking red is only a reason for saying "red" in response to the question "What colour is that?" for a man who already knows (or who already to an extent knows) the meaning of the word "red" (and, of course, of the question "What colour is that?"). And it was only because we took the baby, when he on certain occasions uttered the word "red", having been asked the question "What colour is that?", to be responding (rather than merely reacting) to that question that we were taking him to know (or to that extent to know) the meaning of the word "red". So this has taken us round in a circle.

So I want to draw the following conclusion. *If* it happened that we waved a bit of birch bark over a baby's head, and directly following this his behaviour began to be of such a sort that we could no longer refuse to allow of him that he does indeed have the use of the word "red"—I say "directly following this" to indicate that this only happened when we tried birch bark, and didn't happen when we tried bits of bark from all other kinds of tree we tested—*if* such a thing happened, then something has happened which can have no adequate explanation. All we can say of it is: it happened.[2]

Now it may be that you do not hold it to be analytically true to say that an event is only possible, only conceivable, that it could only happen if it could (somehow or other) be provided with an adequate explanation. You may wish to insist that, inexplicable or not, such things could happen since I can imagine them to happen, or picture them happening. Nevertheless, they are quite different from the mere ordinary run-of-the-mill improbabilities of the sort: the moon is really covered in green cheese. For in the latter cases, though we may not be able to explain what could have produced the effect that the moon is covered in green cheese, the fact remains that the possibility of finding an explanation is *not* ruled out by the description of the situation in the way in which it is ruled out in the case we have been considering. To borrow some phrases from a paper on another subject by Mr. Brian O'Shaughnessy,[3] if such a thing happened as is described in Case R which we have just been considering, this would be "a wound in the side of knowledge". And again, "Such a situation just cannot be faced".

[2] Similarly, even if it should turn out that some man, say Smith, could get physical objects to move whenever and however he wished them to, and this not via his pushing or pulling them, or using magnets or electricity or anything else that we can find as a means of getting them to move, we shall not accept as an adequate explanation of this: "Their movements aren't caused; it is for a reason that they move—namely in order to fulfil Smith's wishes." Smith's success is no more explained by an account in terms of reasons than the baby's is by an account in terms of causes.

[3] "The Origin of Pain", *Analysis*, June 1955

So it seems to me that Hume's maxim concerning what must be present in the course of the process of acquiring the use of names of simple properties is far from a simple empirical claim; it seems to me that the claim "There are no actual situations of the sort described in Case R" is far from a simple empirical claim. We don't want merely to say that such things don't happen, but more, that they somehow couldn't. Such situations just could not be faced.

And if we want to rule out cases of the sort R, we will also want to rule out cases of the sort Q, in which the baby was born, and then, with no teaching, no waving of bits of birch bark over its head, no anything at all, we find its behaviour such as no longer to leave it open to us to deny that he does indeed have the use of the word "red". For explaining *how* such a thing could be so will involve explaining how it could have come about that the baby now behaves in this highly complicated fashion. But all that is available to us as an explanation of this is, once again, some causal account in terms of light rays and brain configurations.

3. Another sort of case which we will want to rule out is S: we point to Smith who has just sniffed at an apple and say to a child "That man just sniffed at an apple"; we point to Jones who has just sniffed at an apple and say to the child "That man just sniffed at an apple"; and so also for Brown, Black, and others who have just sniffed at apples. Some time later, the child sees Gray sniffing at an apple, and then, when Gray puts the apple down, the child announces, pointing to the apple, "It's an apple". And amazingly enough, from here on, though he has never seen an apple before in his life, or been given any discursive definitions of the word "apple", he shows just such behaviour with the word "apple" as no longer to leave it open to us to deny that he does indeed have the use of the word "apple".

How can we explain the fact that the child recognized that first apple he saw as an apple? Suppose we ask him how he knew, and he replies: "Well, you told me that Smith, Jones, Brown, Black, and the others had just sniffed at apples; I could see that *that* object was just sniffed at by Gray—so of course I knew that it was an apple." Even if Gray had on his face the same expression which Smith, Jones, Brown, Black and the others had had on theirs this won't do as an explanation of the child's knowledge—surely Gray might have had just that expression on his face after sniffing at a grapefruit.

Let us compare this with Case A. Case A ran as follows: we take a strip of paper on which is painted a continuous band of

shades of blue from very light to very dark, and we cut out a certain specific small bit of it, and we construct a broken band of the two remaining bits. We show this to a man who has never seen anything of the shade missing from the broken band, and we tell him "The shade boole is the shade which, if inserted in the appropriate place, would result in a continuous band of shades"; he is then able to pick the boole-coloured card out of the batch of colour-cards we then show him.

Suppose, now, that we were to ask him how he knew that the card he picked was the boole-coloured card, and suppose that his reply is: "Well, I could see that this card was of just such a shade as would, if its colour were duplicated in the empty space *there* [he points to the space in the broken band], turn it into a continuous band of shades; so of course I knew that this card was boole-coloured." Now it seems to me that *if* certain other things are true of this man—my reason for introducing a qualification will come out later—I can perfectly well understand this as an explanation of how he could have known which card in the batch of colour-cards was the boole-coloured card.

The difference between these two cases is, it seems to me, this: from the fact that a certain object stands in the relation "was just sniffed at by" to a given man, it does not follow that the thing is an apple—hence I cannot understand how a man could *know* that a thing is an apple, given that all he knows is that it stands in the relation "was just sniffed at by" to a man—even given that he can see that this man's expression is like the expressions of others who he was told stood in this relation to apples. But on the other hand, from the fact that an object stands in a certain relation which I shall call the relation "colour-completes" (defined as follows: "would, if its colour were duplicated in the appropriate space in a broken band, result in a continuous band of shades") to an object of a certain sort (the broken band we constructed by removing a certain specific bit of blue from a band of shades of blue—or any broken band constructed in exactly the same way, i.e., by removing that same specific bit of blue from a band of shades of blue[4]) it *does* follow that the object is boole-coloured—for this is how we introduced the word "boole" into the language.

If we generalize this difference, we can get at a general description of a class of cases which includes Cases A through D

[4] This addition (i.e., the "or" clause) is necessary if "boole" is to stand for a shade of blue. For the broken band I constructed yesterday may have faded by today, and if this addition were not made, something properly called "boole" yesterday would not be properly called "boole" today (for it now no longer colour-completes the particular broken band I constructed yesterday) despite the fact that *its* shade has not changed

and perhaps others as well—that is, a description of a class of cases all of which violate what we called the general maxim, the maxim that all words must be learned either by ostensive definition, or by discursive definition into other words which are themselves ostensively defined. Every such case is of the following sort. Some word "X" is acquired by some person without his being shown any X's or given a discursive definition of the word "X"; what *is* done for him is, instead, this: something (a Y) is pointed out to him, and he is told "An X has the relation R to that object". It may be that this is done several times, pointing to several different objects (Y's) to which an X is said to have the relation R. But in any case, this will work, or better, we shall be able to understand how this teaching procedure could work, *only if* the following is true of X, Y, and R. A thing is an X if and only if there could be something to which it stood in the relation R and which was a Y; a thing is a Y if and only if there could be something which stood to it in the relation R and which was an X; that a thing is an X does not follow from the fact that there could be something to which it stood in the relation R; that a thing is Y does not follow from the fact that there could be something which stood to it in the relation R. I shall abbreviate this condition in the words "only if the relation R between an X and a Y is an *internal* relation between them".

Two points about this usage of the expression "internal relation" are worth mentioning here. In *one* of the senses in which Moore (in his very obscure essay on "External and Internal Relations" in *Philosophical Studies*) uses this expression, "has for a spatial part" is an internal relation between two objects so related—e.g., a table and a table-leg. But this relation between these two objects is clearly not an internal relation between them in my sense: from the fact that a thing contains a table-leg as a spatial part it does not follow that it is a table. Another sort of example (this is not from Moore): the relation "father of" which a certain male has to a certain child is an external relation between them: from the fact that there could be a male which stood in the relation "father of" to a thing, it does not follow that that thing is a child (it might be an adult)—and also, from the fact that there could be *something* (child, adult, . . .) to which a given thing stood in the relation "father of" it does follow that that thing is a male. Point (2) about my usage of the expression "internal relation" is this: one cannot ask of some relation whether or not it, pure and simple, is an internal relation. For example, the relation "would fit smoothly into" (or, if you prefer, "smoothly space-completes") which a certain sphere may have to a certain block is an internal relation between them; never-

theless the relation "smoothly space-completes" which a certain red rubber ball has to a certain block is an external relation between them—from the fact that there could be a block which a given object space-completed it does not follow that that object is a red rubber ball. So in my use of the expression "internal relation", one can only ask whether or not a relation R which one object may have to another is an internal relation between them if descriptions "X" and "Y" of these objects are specified. It is only between objects-under-descriptions that relations are either internal or external.

Not only is the relation "smoothly space-completes" which a sphere has to a block an internal relation between them, but so also is the relation "colour-completes" which a boole-coloured object has to an object of the sort indicated in Case A (a broken band constructed in a specific way) an internal relation between them. However the relation "was just sniffed at by" which an apple may have to a man is not an internal relation between them: from the fact that there could be a male to which a given object stood in the relation "was just sniffed at by" it does not follow that that object is an apple.

To sum up. I said that we can describe a class of cases, all of which violate the general maxim; these cases will be admissible, they will describe admissible situations *only if* the relation by means of which the word "X" is taught is an internal relation between an X and an object of the sort indicated in the course of the teaching. I say "only if" because it may be that we shall want to rule out a case in which the word "X" is supposedly taught by the use of an internal relation on other grounds. I should suppose, for example, that we would wish to rule out the possibility of Case A if the learner in question does not know the meaning of the expression "continuous band of, or progression of, shades". If he has never before met a sample of a continuous band of shades, I think we should find incomprehensible, inexplicable, his ability to recognize that if the shade of this particular object were duplicated in that space (the space in the broken band) the result would be a "continuous band of shades". How could he have known that this would be the result if he does not know what this is? Or again, how could a man have known that he may say "This given object would fit smoothly into that one", unless he had somewhere before been introduced to the meaning of "would fit smoothly into"—unless he has at some time seen something being put into something else and fitting smoothly into it, and been given this as a description of what was happening? But one can have acquired the meaning of "a continuous band of shades" without ever having

seen a continuous band of shades of *blue*; and one can have acquired the meaning of "would fit smoothly into" without ever having seen a *sphere* being inserted into and fitting smoothly into a block. So that it seems to me that, whatever else may have to be true of a man if a description of a case in which a man is supposedly taught the meaning of the word "X" by the use of an internal relation is to be a description of a possible, admissible situation, this much remains true: the learner need never have seen a sample of the kind X, and need never have been given a discursive definition of the word "X".

I am going to call the process of teaching the meaning of the word "X" in the manner indicated "definition by internal relation" to mark the fact that the relation R by means of which the word "X" is taught must be an internal relation if we are to understand how the teaching process could have succeeded. And I think we can say: in *some* cases we can understand how the process could have succeeded, despite the fact that the learner never in the course of it or at any time in the past saw a sample X, and never was given a discursive definition of the word "X".

4. Definition by internal relation is different from what customarily passes under the name "definition". No one, I suppose, would be tempted to claim that what we are concerned with here is the familiar ostensive definition—in definition of the expression "X" by internal relation, no sample X is pointed out to the learner. But it may be said that definition by internal relation is really only an elaborately described combination of ostensive and discursive definition. One may say that what is really done in defining the word "sphere", say, by internal relation as above is first, ostensive definition of the word "block", and second, discursive definition of the word "sphere" as follows: the statement "x is a sphere" means the same as the statement "x would fit smoothly into a block". (Or rather, to be more precise, since a thing can be a sphere even if there is no block into which it does fit smoothly—the supply of blocks is not inexhaustible [as it would be if these were geometrical concepts], and all the existing ones may be too large or too small—"x is a sphere" means the same as "There could be a y which was a block and into which x fitted smoothly".)

But this is a mistake. I may of course define the word "sphere" in this way, but if I do I am not defining it by internal relation. In actually defining the word "sphere" by internal relation as above I never once use the word "block"; all I do is point to one block after another, saying of each "A sphere would fit smoothly into that". The word "block" appears in this paper

only because I needed a general term by means of which to refer to the things I point to. But in defining the word "sphere" by internal relation as above, all I do is point to one block after another, saying of each "A sphere would fit smoothly into that". And it seems to me that the fact that I do not first ostensively define the word "block" is no bar to the possibility of a man coming to be able to identify a thing as a sphere even where it does not fit smoothly into some one of the actual set of blocks to which I pointed in the course of the teaching. It seems to me that I would have no more difficulty in understanding how a man could know that this given object would fit smoothly into an object *of the sort* indicated—though perhaps only into one larger or smaller than the objects so far indicated—it seems to me that I would have no more difficulty in understanding how a man could know *this* than I would have in understanding how a man can know as a result of a sequence of pointings to a set of objects, say apples, rubber balls, and tomatoes, that some given object is also red, even where the given object is neither an apple nor a rubber ball nor a tomato, but, say, a bit of silk. In other words, in both ostensive definition of the word "red" and definition by internal relation of the word "sphere" the learner has to be able to step from the particulars to the universal, and it seems to me no more difficult to understand how this step can be taken in the case of the former than it is to understand it in the case of the latter.

I want to say that in definition of the word "X" by internal relation there are not *two* processes, first ostensive and second discursive definition; that there is only *one* process, which is, like the process of ostensive definition, a process of pointing and labelling—though the object indicated is not labelled "X", but rather the following is said of it: "An X has the relation R to that". One may wish to insist, however, that we should speak of two processes here, on the ground that, if the thing is to work, the learner must in some sense acquire the concept "Y" (e.g., "block") if he is to acquire the concept "X" (e.g., "sphere"), for he must know that *this* is an X (a sphere) even though it does not have the relation R (fitting smoothly into) to any of the Y's (blocks) we pointed to in the course of the teaching. But the "in some sense" is crucial here; as I have said, we need not give him the word "Y" ("block"), or any synonym of "Y", and it is far from clear what could be meant by "acquiring the concept 'Y' though not under the description 'Y', or indeed under any description at all". It seems to me that the desire to say "We *must* speak of two processes here rather than just of one process" arises out of nothing more than a blind refusal to

consider interpreting the meaning-structure of a language in accordance with any other model than that presented in what I have called "the general maxim".

5. In sections 3 and 4, the point was made that there is a mode of definition, which I call "definition by internal relation", which is different from what commonly passes under the name "definition". What I want to turn to (very briefly) here is *one* suggestion as to why it is worth while making this point. It will be no more than a suggestion.

If we turn back again to the two maxims with which we began, we can now see that if the preceding arguments are correct, the one which I have called the general maxim is falsified by the existence of the possibility of defining words by internal relation. If the preceding arguments are correct, it is simply not the case that all words must be either ostensively defined or reduced by discursive definition to words which are ostensively defined if their meaning is to be taught.[5] But I regard the fact that *this* maxim has been falsified as of no importance whatever—except that of reminding us of the dangers of clichés in philosophy.

But the question whether or not the possibility of defining terms by internal relation falsifies *Hume's* maxim I do not regard as of no importance.

What I wish to suggest is that despite the fact that words such as "red", "blue", and the like can be defined by internal relation, despite the fact that they need not be defined ostensively if their meaning is to be taught, it does *not* follow from this that Hume's maxim has been falsified. What I suggest we should say is this, that if a man has learned the meanings of the words "red", "blue", and the like by internal relation, then in his mouth, these words are not names of simple properties.

Of course one can if one likes say that words like "red" and "blue" are by definition names of simple properties—that is, that what we mean by the expression "name of a simple property" is just this: words like "red", "blue", . . .—and so it follows that Hume's maxim has been falsified. But this would be an unfortunate suggestion; we can use words in whatever way we like, and I suppose that no one would wish to claim that because I can privately decide to use the predicate "is red" to mean "is a hard rubber building block" it follows that being a hard rubber building block is a simple property.

"Well", it may be said, "I didn't mean that the *words* 'red' and 'blue' are necessarily names of simple properties. Of course

[5] I am not suggesting that the general maxim is correct if emended to allow for the possibility of definition by internal relation, but only that it is not correct if not so emended.

they can be used to refer to other than simple properties if we wish to do so. What I meant was the words in a certain *use*, the words as used to refer to the things they are in fact used to refer to. Now though *you* may have acquired the word 'red' by definition by internal relation, you presumably still use it to describe the same objects; you apply the adjective 'is red' to the same objects to which a man applies it who acquired it ostensively. So as you apply it to the same objects you refer to the same property. So you *both* refer to a simple property. So the names of simple properties can be acquired without ostensive teaching. So Hume's maxim is false."

What appears here is just what I want to get at. It is customary in certain circles nowadays to insist (following Wittgenstein) "Don't ask for the meaning of an expression, ask for its *use*!" Now I am not quite sure what Wittgenstein himself meant by this imperative. At times (most strikingly in ethics, but elsewhere as well), it has been interpreted to mean: don't ask for the "cognitive" content of an expression, ask for its "non-cognitive" functions (prescriptive, performatory, regulative, . . .). (One thinks here also of the primitive language games at the beginning of the *Philosophical Investigations*.) And at other times it has been interpreted to mean: don't ask what *property* a set of things have in common, their joint possession of which makes them be of the kind they are; ask instead to what things we do in fact apply the word. (One thinks here of the influence of Wittgenstein on the concept of a game, and of what is perhaps even more important, the influence of G. E. Moore.)

But whatever Wittgenstein meant by the imperative, the fact remains that there may be differences between two expressions, both of which apply to the same sets of objects (even, "same sets of actual and possible objects") and both of which serve the same non-linguistic purposes. I offer as an example "clear green$_1$" whose meaning is given in the usual way by ostensive definition, and "clear green$_2$" whose meaning is given by definition by internal relation; both may apply to the same sets of objects, and both may serve the same non-linguistic purposes, and yet a man who has been taught "clear green$_1$" has a different reason for applying this expression to a given object (the given object is like those and those) from that which a man who has been taught "clear green$_2$" has for applying *this* expression to a given object (perhaps: the given object is of just such a colour as would, if duplicated in that space there, produce a continuous band of colours). The *use* (in the sense indicated) of these two expressions is the same, but the reasons for applying them are different.

Furthermore it seems to me that in the case of some words

(though perhaps not in the case of "red", "clear green", . . .) this may be an important difference. For I take it that it is by now clear to all of us that the familiar old problems of philosophy have not been eliminated by the finding out of the manner in which the troublesome word is used (its "non-cognitive" role), and by simply calling attention to the things to which people do in fact apply it. The old problems have not been solved in this way because they do not arise out of doubt as to these things; many, I think, arise because of some difficulty in, some puzzlement about, the nature of the concept itself—which only *then* results in doubt as to the manner in which it is "fundamentally" used (its "non-cognitive" role), and hesitation over when to apply it, even in situations where there was previously no doubt and no hesitation. And if this is so, then unless "use" is interpreted more broadly than it has been, obeying the imperative "Don't ask for the meaning, ask for the use!" only keeps us from getting at the source of the problems.

So what I am urging when I suggest that the possibility of defining terms like "red" by internal relation does not falsify Hume's maxim is this: that we should regard more than use (in the sense indicated) as relevant to the meaning of a term. In fact that it may prove useful to consider also the kind of reasons a man who is said to know the meaning of a term has for applying it, and for applying it in the particular manner in which he does.

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THEOLOGY AND FALSIFICATION AGAIN

By D. R. DUFF-FORBES

In a well-known paper[1] Professor Flew puts forward the claim that theological utterances are not literally and factually meaningful, contrary to all appearances, because they are not falsifiable, nothing will count against them, no observation statement is relevant to the determination of their truth or falsity. Flew confronts the religious man with the challenge, "Do you or do you not allow that anything can count against your religious assertion? If you reply in the affirmative then I shall agree with you that you are making a factual statement. But I shall require you to tell me what *would* be accepted as counting against your assertion. If on the other hand you reply in the negative, isn't it the case that because your utterance excludes no possible state of affairs it succeeds in asserting nothing?" Flew, clearly, maintains that the latter situation obtains in the case of theological utterances and hence they are not to be treated as factual assertions.

It is evident that the starting point of Flew's argument is the assumption that religious utterances, or *some* religious utterances, are factual statements, that they do make assertions that such-and-such is the case, do describe and make reports about the world. This is not to say that *all* religious utterances are such; petitions, recommendations, commands, exhortations all play important roles in religious language. But there can be little doubt that many religious utterances are put forward as assertions and taken as such; it is with these that Flew is concerned. And the crux of his case is simply this: "an assertion, to be an assertion at all, must claim that things stand thus and thus; *and not otherwise*".[2] To assert that anything is the case is to claim that certain alternatives are not the case; its value as an assertion lies in its ability to exclude certain possibilities, to place a limit upon expectations by ruling out certain situations. It is Flew's complaint that religious utterances do not comply with these requirements and hence cannot legitimately be regarded as specifying anything about the state of affairs with which they purport to deal.

That is the case for the prosecution. And what of the defence? I propose to consider three replies to Flew; the first concedes that the religious utterances in question are to be taken

[1] Antony Flew and Alasdair MacIntyre (eds.) *New Essays in Philosophical Theology*, pp. 96-99, 106-108.
[2] *Ibid.*, p. 106.

in ways other than as assertions, whilst the second and third offer a defence of their status as factual statements.

I

The first reply consists of the position advocated by R. M. Hare[3] as a counter to Flew, *viz.* the famous case of religion as a "blik" and not a system of assertions.

Hare makes it quite clear that he does not regard religious utterances as factual statements; he commences his reply by conceding "that, on the ground marked out by Flew, he seems to me to be completely victorious".[4] But it is, nonetheless, a hollow victory according to Hare, a victory simply "on the ground marked out" by the attacker. If this is to be taken as a suggestion that Flew has simply sighted his guns on a target of his own making and hence *of course* he scores a hit, then I think Hare is unfair to his opponent. Because, as I have said above, it seems beyond question that religious utterances are frequently (perhaps more often than not) put forward as factual assertions, and Flew, as I see it, is completely in the right when he replies, "I simply do not believe that they are not both intended and interpreted to be or at any rate to presuppose assertions . . ."[5] Of course, it is open to Hare to say that such a position is mistaken, and this he proceeds to do. Religious utterances are not assertions; on the contrary, they give voice to what Hare calls a "blik", an attitude to life, "a confirmed and habitual way of looking at things".[6]

Hare sets the field by recounting the case of a lunatic who is obsessed with the notion that all dons are plotting against his life. No matter how many dons he meets of mild and gentle demeanour and no matter how often it is urged by his friends, "You see, he doesn't really mean you mischief; see how pleasantly he spoke to you", the graduate's reply is the same, "Yes, but that was only his diabolical cunning; he's really plotting against me the whole time, like the rest of them". In fact no situation, no state of affairs is accepted as counting against the graduate's theory about dons; it is unfalsifiable and hence succeeds, contrary to appearances, in asserting nothing. But, Hare points out, "it does not follow that there is no difference between what he thinks about dons and what most of us think about them"; the difference is a difference of *respective* "bliks", one of which is right and the other wrong.

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[3] R. M. Hare, "Theology and Falsification", *New Essays in Philosophical Theology*, pp. 99-103.
[4] *Ibid.*, p. 99.
[5] *Op. cit.*, p. 108.
[6] A. Boyce Gibson, "Modern Philosophers Consider Religion", *Australasian Journal of Philosophy*, Vol. 35, No. 3, p. 171.

It is important to note this. Hare is not saying that the *lunatic* alone has a "blik" about dons; he is saying that *both* the lunatic *and* those who disagree with him about dons have "bliks", one is an insane "blik", the other a sane "blik", one is wrong and the other right. It is not a question either of having or not having a "blik", but a question of having either a wrong or a right "blik". The point is you *have* one, and it is important to have the right one. Clearly, Hare is maintaining that the religious man is not peculiar, not alone in having a "blik" about the world. All men have "bliks"; again, the important thing is to have the right ones. And equally clearly Hare holds that the religious man's "blik" about the world *is* the right one.

I stress this, because it seems to me that Hare's point here is frequently overlooked. For example, Professor Passmore remarks, "The analogy between the religious believer and the insane graduate might be an uncomfortable one for Hare to press hard".[1] But the analogy, it so happens, is *not* between the religious believer and the insane graduate at all; the religious believer's "blik" is a right one, the insane graduate's is a wrong one. The analogy is between the religious believer and those who held that the graduate was insane. I have a further reason for stressing this point; not only because it is frequently overlooked, but because, surprisingly, Hare, in my opinion, comes to grief upon it.

For consider: Hare says that the lunatic will accept nothing as counting against his theory about dons; hence it is unfalsifiable and because of this it is not an assertion but a "blik", and an insane one. Those who differ from the lunatic also, Hare holds, have a "blik", but a sane one. Hare wishes to insist that *all* have a "blik" about dons, but that one "blik" is the right one, the other the wrong one. Hence he marks the difference by saying that one is insane, the other sane. But on what grounds is one said to be insane and the other sane? Surely this, that the graduate holds to his theory in the face of any and all observations that can be made concerning the behaviour of dons (this is why he is said to be a lunatic), whereas his friends *do* allow the behaviour of dons to count. And if the behaviour of dons had been other than it was (if dons had been creeping around after dark with guns and knives and pots of poison), then the opinion of the lunatic's friends would have been other than it was. This is why they are said to be sane. But then what *they* say about dons is falsifiable, and if *that* is so, then they, unlike the lunatic, haven't a "blik" about dons at all. They make factual claims. The right "blik" turns out not to be a "blik" at all.

[1] J. A. Passmore, "Christianity and Positivism", *Australasian Journal of Philosophy*, Vol. 35, No. 2, p. 130.

This, however, is not in the least what Hare wants. He wants, as I have said, to say that the religious man is not alone in having a "blik", and that the question is not one of having or not having a "blik" about the world, but a question of having the right one. But unfortunately Hare's story of the lunatic does not succeed in showing this. If *both* the graduate *and* his friends have a "blik" about dons, that is to say, if the theories of *both* are compatible with anything and everything that can be observed about the behaviour of dons, then no state of affairs can count as between them. But then the ground for saying that one is a sane "blik" and the other an insane "blik" has gone. The language of "lunatic", "obsession", "insane" used of the graduate is simply the language of *our* "blik", as the language of "simple-minded", "stupidly trusting" and so on, used of us by the graduate, is the language of *his* "blik". Hare has robbed himself of the ability to draw a distinction between the right and the wrong "blik". Or, at least, if he draws the distinction in the way that he does he is left with only *one* "blik", the lunatic's, and if he insists that *both* the lunatic and his friends have "bliks" about dons, he is deprived of the distinction he requires. Hare cannot, it seems to me, hold that *all* have "bliks" *and* say that one is better than another, one is right and the other wrong; or if he can hold this, he has not shown that he can. Nevertheless it is important to realise that this is what he wishes to maintain.

I suspect that the same situation confronts us in his second example, the case of "bliks" about steel and its properties. The theory about steel which holds that it is utterly untrustworthy, can never be relied upon, that cars are therefore murderous instruments, that one's life is in perpetual jeopardy whilst in one, and so on, qualifies, clearly, as a "blik". As Hare points out, "No amount of safe arrivals or bench tests will remove my *blik* and restore the normal one; for my *blik* is compatible with any finite number of such tests".[8] Nothing will count against it, it is unfalsifiable. That is to say it *is* a "blik".

But what of the "normal blik" about steel and its reliability? Hare nowhere suggests that this *also* is compatible with any and all tests and any and all observations about arrivals *or non-arrivals*. *This* "blik", it would seem, is based on observations of safe-arrivals etc. etc., which is also to say that the break-down of tests and the persistent failure to arrive safely would count against it. But then, as in the lunatic example, what Hare clearly regards as the right "blik" is not a "blik" at all. He gives his case away when in answer to the question, "How do I know that this (the

[8] *Op. cit.*, p. 101.

parting of steel joints or the breaking of steel rods and so on) won't happen?" he answers, "The truth is, I don't know; I just have a *blik* about steel and its properties, so that normally I trust the steering of my car".[9] Why "*normally* I trust the steering of my car"? To say "normally" is to say "in most cases". But what of the other cases in which the steering is not trusted? Doesn't this suggest that in such cases some state of affairs that would count against the reliability of steel has been taken into consideration, has been allowed to count? But then what is said in this case about steel and its reliability is falsifiable and does not, after all, give voice to a "blik". Again, the normal "blik", the right "blik" turns out not to be a "blik" at all.

What has happened to the notion of a right and a wrong "blik" in any given context? I can only conclude that Hare has not succeeded in establishing his case Perhaps, after all, and contrary to Hare's insistence, it *is* a question of having or not having a "blik", and *not* a question of having the right one and not the wrong one. At least Hare has not succeeded in showing that this is not the question. Unfortunately for his contention, against Flew, that religious utterances are not to be taken as purporting to make assertions but on the contrary are to be taken as giving voice to a "blik", the only "bliks" that remain with which to draw a parallel are those of the lunatic and the man who is "silly" about steel. And *this* is the moment to make Passmore's remark about the embarrassment of an analogy between the religious believer and the insane graduate. It is not, however, the parallel that Hare intended.

II

The second case for the defence is advanced by Basil Mitchell.[10] Unlike Hare, Mitchell purports to meet Flew head on. He is not prepared to concede, as Hare had conceded, that Flew is "completely victorious" in the chosen area of discussion. He maintains that his case differs from Hare's and adds, "This means that I agree with Flew that theological utterances must be assertions";[11] by which, I take it, Mitchell wishes to insist that the only genuine defence of theological utterances is a defence of their status as factual assertions.

Mitchell, as I have said, endeavours to meet Flew's objection to theological utterances head on. He counters that there is "something odd" about the way Flew has represented the

[9] *Ibid.*, p. 100.
[10] Basil Mitchell, "Theology and Falsification", *New Essays in Philosophical Theology*, pp. 103-105.
[11] *Ibid.*, p. 105.

theologian's position. Flew had argued that the religious man will not allow that anything could count against the assertion "God loves us as a father loves his children". This, Mitchell alleges, is simply false. The religious man *does* allow states of affairs to count against this assertion, for example, the fact of pain. "But it is true that he will not allow it—or anything—to count decisively against it; for he is committed by his faith to trust in God".[12] The theologian, then, allows states of affairs to count against, but will not allow that *any* state of affairs, *ever*, could count decisively against. He is 'precluded by his faith" from adopting the attitude that assertions of the sort "God loves men" are "to be discarded if experience tells against them". Quite to the contrary; they are "significant articles of faith".[13]

Mitchell supports his case by the story of the resistance-fighter who because of a meeting with a Stranger is convinced that the Stranger is on the side of the resistance. Subsequently the behaviour of the Stranger is irrelevant to the belief that the partisan has; no matter what takes place the partisan insists, "He is on our side". Even when the evidence is most damning the partisan still maintains that, contrary to all appearances, the Stranger has not betrayed him or his colleagues. So, Mitchell argues, "The partisan of the parable does not allow anything to count decisively against the proposition 'The Stranger is on our side'. This is because he has committed himself to trust the Stranger. But he of course recognizes that the Stranger's ambiguous behaviour *does* count against what he believes about him".[14]

What plausibility Mitchell's argument possesses rests to a considerable degree upon the way he uses the words "count against" and "count decisively against". The partisan *does* allow that certain actions of the Stranger may and do count against the assertion, "He is on our side"; the religious man *does* allow that certain states of affairs may and do count against the assertion, "God loves us as a father loves his children". And this has a reassuring air about it: theological utterances *are* factual assertions, certain states of affairs *would* be accepted as counting against them, they *are* falsifiable. The task before the would-be critic, it now seems, is that of looking and seeing. But if he concludes from Mitchell's remarks that this is the case then he is sadly misled. To *every* potentially falsifying state of affairs that he advances the answer is the same:

"I freely admit that what you say counts against my statement."

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[12] *Ibid*, p. 103.
[13] *Ibid.*, p. 105.
[14] *Ibid.*, p. 104.

"Then you withdraw it? You admit that God does *not* love men after all (or, the Stranger is *not*, after all, on our side)?"

"Not at all. I admit no such thing What you say counts against my assertion, certainly, but it doesn't count decisively."

"I see. You hold that the state of affairs I pointed to is not *sufficiently conclusive*. I see I shall have to do better if I am to convince you that the step from *suspending* judgment to *passing* judgment should be taken."

"No, you have misunderstood me again. There *never* can be such a thing as a state of affairs that is sufficiently conclusive. Such a consideration can never arise. There *never* can be such a step as the step from suspending judgment to passing judgment, for the simple reason that judgment is never suspended. God *does* love men (the Stranger *is* on our side); that can *never* be called in question. There is no point whatever in asking what *would* convince me, what *would* count decisively against, for the simple reason that *nothing* would, under any circumstances, at any time. I am precluded by my faith from adopting such an attitude."

But what has become of the religious man's original admission that such-and-such a state of affairs counted against the assertion "God loves men"? Hasn't it changed its significance in the course of the argument? Aren't we justified in alleging that his admission is now no admission at all; or, at least that if an admission *was* made it certainly was not an admission that the state of affairs in question counted (decisively, indecisively, or in any other way) against the assertion, "God loves men"? If an admission was made it certainly was not an admission that the state of affairs in question *appears* to count against the statement that "God loves men", and this is *always* followed by the rider "but of course it doesn't *really* count against it at all".

Mitchell, I suspect, has created a verbal illusion, the illusion that the question to whether or not God loves men is an open question, that states of affairs and observable situations are relevant to the determination of its truth or falsity, that states of affairs *count*. In fact they never *do* count. By the use to which he puts the expressions "count against" and "count decisively against" Mitchell creates the illusion that there is only a step from one to the other, the step of making the crucial observation ("This settles it, this puts the issue beyond doubt; it is *not* the case that God loves men"). But in fact there is *no* such step and there is *no* such crucial observation; the admission that such-and-such a situation "counts against" the claim that God loves men is the last step the religious man *can* take, because, as Mitchell uses it, it says no more than "such-and-such a situation appears to count against the claim that God loves men but in fact it doesn't". If

the religious man concedes that such-and-such a state of affairs counts against his claim that God loves men, but, because the statement "God loves men" is an article of faith, allows that it counts against *only* in the sense that it appears to count against (but really doesn't), and if the religious man further maintains that no conceivable state of affairs could count other than in the sense that it appears to count against (but really doesn't), isn't it perfectly obvious that states of affairs and observable situations are irrelevant, that they never *do* count *in the required sense*?

Mitchell is paying lip-service only to the logical requirements of assertions; the "counting against" he admits to is not counting against in the required sense. When Flew, for example, challenges, "Do you or do you not allow that anything can count against your (religious) assertion?" he is *not* asking "Do you or do you not allow that anything can *appear* to count against your (religious) assertion?" To *this* Mitchell can reply (as he does) "Yes, pain can and does". But he has not in the least met Flew's challenge. To the question, "Do you or do you not allow that anything can count against your (religious) assertion?", i.e., falsify it, Mitchell must reply, "No, because the statement in question is an article of faith". I can only conclude that Mitchell has not succeeded in showing that the theological utterance in question qualifies as an assertion. It is unfalsifiable, compatible with any and every state of affairs, it excludes no possibilities; hence it is not a factual, true-or-false statement.

I suspect, however, that worse is to follow. Return for a moment to Mitchell's parable about the resistance fighter and the Stranger: if the partisan's persistent claim "The Stranger is on our side" is not, in spite of Mitchell's protests, a factual statement (because unfalsifiable), but nonetheless is something the partisan always holds to (it amounts to a creed with him), in what way does the partisan's attitude to the Stranger differ from the lunatic's "blik" about dons?

Consider Mitchell's parable again, but with this difference: we are no longer misled by his use of "counts against" and "counts decisively against". The story he gives is that of a member of the resistance who meets a Stranger, and with no previous knowledge of the Stranger nor an opportunity to observe him in test situations commits himself without question or reserve to the creed "The Stranger is on our side". From that point on no situation, no state of affairs is accepted by the partisan as counting against his theory. The most damning situations can be pointed to, but the partisan still holds to his attitude to the Stranger: "He is on our side. He is only testing us, or strengthening us for greater endurance", and so on. *Nothing* that occurs

and nothing that *can* occur falsifies his claim about the Stranger, hence, contrary to appearances, when he says, as he does persistently, "The Stranger is on our side" he succeeds in asserting nothing. But then isn't it the case that when he says "He is on our side" the partisan is giving expression to a "blik" about the Stranger?

Mitchell has his answer ready. He claims that his parable differs from Hare's on two counts: "The partisan admits that many things may and do count against his belief: whereas Hare's lunatic who has a *blik* about dons doesn't admit that anything counts against his *blik*. Nothing *can* count against *bliks*".[15] That is his first point. To which I can only reply that Mitchell is wholly mistaken. The fact is that Hare's insane graduate *does* allow that certain things count against his "blik", and what's even more to the point, he allows that they count against his "blik" in the same way that the partisan allows certain things to count against *his* belief about the Stranger. That is to say *both* the lunatic *and* the partisan allow that certain states of affairs count against their respective theories, but only in the sense that they *appear* to count against, but in fact don't. I have already shown this to be the case with the partisan; clearly it is also the case with the insane graduate and his "blik" about dons. When confronted with the evidence of the don's kindly manner, the lunatic replied, "Yes, but that was only his diabolical cunning; he's really plotting", and so on. *Of course* he allowed that the don's manner counted against his theory, but only in the sense allowed by Mitchell's partisan, *viz.* that it *appeared* to count against, but really didn't. The situations are identical; states of affairs can and do count against "bliks", but only in the sense described. Indeed this is a characteristic of "bliks", and it is as much a characteristic of the attitude of the partisan as it is of the attitude of the lunatic.

Mitchell's second point, I wish to claim, is equally wide of the mark: "Also the partisan has a reason for having in the first instance committed himself, *viz.* the character of the Stranger; whereas the lunatic has no reason for his *blik* about dons—because, of course, you can't have reasons for *bliks*".[16] Again, I think, Mitchell is wholly mistaken. Whether or not one would wish to call any justifying account advanced by the insane graduate a "reason" I do not know, nor, for all I can see, does it much matter. What does matter is this: the lunatic has no less and no more a reason for *his* "blik" about dons than the partisan has for his attitude to the Stranger. If pressed to account for

[15] *Ibid.*, p. 105.
[16] *Ibid.*, p. 105

his "blik" isn't it reasonable to suppose that the lunatic would advance as a reason his knowledge of the character of dons? And wouldn't he claim that he knew a great deal more about this than anyone else, indeed, had unique insight into the character of dons? This, surely, is the whole point of his refusal to allow any states of affairs to count against his theory about dons. I can only conclude, then, that Mitchell also has failed to establish his case.

III

One further defensive measure remains for consideration, and about this I shall make one or two more or less inconclusive observations. In a much more complex argument than I can take note of here, I. M. Crombie,[17] like Mitchell, advances the claim that religious utterances are to be logically classified as statements of fact. In justification of this claim, Crombie takes a leaf out of the books of certain logical empiricists,[18] and holds that whilst the assertion that God is merciful, or God loves us, cannot be falsified in fact, in practice, nonetheless it is falsifiable in *principle,* and that "the operation of getting into position to decide" whether or not suffering, for example, is incompatible with the claim that God loves us "is called dying". He concludes his argument on this point thus: "By this test, then, religious utterances can be called statements of fact".[19]

This, then, is the move used by Crombie in defence of the factual status of religious utterances; unfortunately, a little reflection will show that his case is by no means as straightforward as his somewhat categorical treatment may lead us to suppose. For consider: the statement that such a test can be applied to religious utterances, enabling us to say they are factual statements, is itself expressed as one of the religious utterances in question. I do not think that it can be doubted that certain utterances purporting to make claims about a life after death are characteristically religious utterances. Crombie clearly regards them as such when he speaks of belief in the world to come as a defensive position for the religious man.[20] The claim, then, that such a test is possible is itself one of the religious utterances ("There is a life after death where all things are made plain") declared factual by this test. Hence in order to establish whether or not claims about life after death are factual, true-or-false statements (because religious

[17] I. M. Crombie, 'Theology and Falsification", *New Essays in Philosophical Theology,* pp. 109-130.
[18] E.g. Schlick and Ayer
[19] *Op. cit.,* p. 126
[20] *Ibid.,* p. 129.

utterances), we must establish that they are *true* statements. But this supposes that they *are* factual statements.

If Crombie manages to extricate himself from this muddle and attempts to apply the "falsifiable in principle" technique to claims about life after death, treating these as belonging in a category of their own (although, as I have said, he suggests no such thing), even then he won't help the situation a great deal. He *may* succeed in showing that claims of this particular sort are factual on the ground that they are verifiable-falsifiable, potentially, in the world to come. But this (if successful) won't get him beyond the *factual status* of this *particular* utterance. The establishment of its *truth or falsity* will depend on waiting and seeing and dying, and then on its being true! But the factual status of religious utterances in general, on Crombie's showing, is dependent upon the *truth* of this one statement. Hence, because, presumably, Crombie was alive and well when he wrote his article, he had no justification for writing, as he did, "By this test, then, religious utterances can be called statements of fact".

It is evident, I think, that the whole drift of Crombie's argument requires that he should treat religious statements as hypotheses, more or less tentative in nature, dependent for confirmation or falsification upon the individual eventually occupying the only available testing position at death. And, of course, much of what he says accords with this. But on the other hand, Crombie also writes in this vein: "Does anything count against the assertion that God is merciful? Yes, suffering. *Does anything count decisively against it? No, we reply, because it is true.* Could anything count decisively against it? Yes, suffering which was utterly, eternally and irredeemably pointless. Can we then design a crucial experiment? No, because we can never see all of the picture".[21] There is little doubt that here Crombie has allowed himself the expensive luxury of a curious blunder. On Crombie's showing, statements of the sort, "God is merciful" or "God loves men", can be regarded, here and now, as *no more* than tentative hypotheses awaiting eventual confirmation or refutation in the world to come; the crucial data are not now available to us and will only be accessible to us in the life to come. Argument, then, that takes place between the religious man, who holds that suffering and pain count only *prima facie* against the assertion that God loves men, and his opponent who holds, on the contrary, that suffering and pain count *decisively* against it, can never be settled; not until the confirming—or falsifying—data are available in the life to come. This is Crombie's clearly stated position, as we have already observed.

[21] *Ibid*, p. 124. My italics.

But for this reason Crombie is precluded from replying (as he does) to the query, "Does anything count decisively against the assertion that God is merciful?" by claiming, "No, because it is true". Crombie's position gives not the slightest warrant for such a reply. So far as I can see Crombie's argument allows for only one reply to the question "Does anything count decisively against the assertion that God is merciful?", and it is certainly not the reply he gives here. *Of course* Crombie's religious man will reply, "No, nothing in fact counts decisively against the assertion that God is merciful"; and he will do so for the simple reason that it is the whole of Crombie's case that the crucial data are not, here and now, open to us, that the conclusive parts of the picture enabling us *either* to refute *or* confirm the assertion are not yet available (but will be in the world to come). And the reason the religious man has for replying, "No, nothing in fact counts decisively against the assertion" is *also* the reason why he cannot add, "because it is true". Clearly Crombie can't, on the one hand, talk about nothing counting *decisively* against religious claims *because in fact they are true,* and at the same time hold to the theory of their factual status that he does.

To conclude: Mitchell and Crombie, endeavouring to defend the factual status of religious utterances, simply provide further examples of the sort of position to which Flew objects, *viz.* the position that holds on the one hand that theological utterances refer to actual states of affairs, embrace some possibilities and exclude others, whilst refusing, on the other hand, to expose these claims to contingent situations. Hare, of course, is exempt from *this* criticism, but fares no better than the others in establishing his case.

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"DISTANCING" AS AN AESTHETIC PRINCIPLE

By SHEILA DAWSON

No cardiologist is professionally interested in Wordsworth's declaration that his heart leaps up when he beholds a rainbow in the sky; nor, save from sheer perversity, do we envisage a thorny wench in the throes of apoplexy when Burns tells us that his love is like a red, red rose. It is, I think, obvious that imaginative and scientific truth are not co-extensive and that intelligence alone cannot illuminate the one as it can the other. It follows that the evaluation of a work of art is not the same kind of operation as the evaluation of a scientific formula; there is no calculation or end-product that can prove my aesthetic judgments to be correct, that is, universally valid.

Nevertheless it is generally agreed that a true aesthetic judgment *is* universally valid. If something is beautiful (as distinct from pleasing in a descriptive sense) it is so intrinsically. That I may not appreciate it, or that I may even actively dislike it, does not alter the fact. There is nothing contradictory about my saying that the 'Mona Lisa' is beautiful, yet I dislike it. The object of appraisal is a single unique and unchanging individual; but *each* appraising person is a unique individual and he, and the public of which he is a member, is constantly changing.

Evidently then to assess a work of art qua work of art is not a simple matter. A popularity poll will tell us more about the current likes and dislikes of a set of people than about the intrinsic merits of the work they vote for; such a judgment will become more stable over a period of time—the label 'classic' usually denotes grain rather than chaff—but in any case whose opinion are we to seek, everybody's or the experts'? (and who is to decide which people are the experts?). The only judgment of which each can be sure is his own—'I now find this beautiful' is definite and irrefutable; I know whether it is true or false, and I am making no assertion about my past or future judgments. (In spite of this qualification, 'I now find this beautiful' is very different from the meaningless 'This is beautiful for me', for the meaningful statement implies 'I now judge this thing to be intrinsically beautiful'.)

Even so I know that I now find things beautiful which some time ago I should have thought uninteresting or bizarre. Knowledge and experience alter our appreciative capacity, we come to think and feel differently about unchanging objects, because we

change in relation to them. This process is, of course, not limited to our appreciation of works of art. Obviously we do not always recognize beauty, how then can we say what it ought to be and to effect? Still I must have some point of contact with a work of art if it is to have meaning and value for me. I therefore suggest that instead of attempting to formulate definitions of abstract terms, as has so often been done in the past, it would be more profitable to study relationships—how initial stimulus (subject), artist, work of art, and audience react with one another.

The success of a work of art depends not only upon its intrinsic value but also upon the attitude of people to it. There are many well-known examples of works of art and of artists who fail to be recognized at first but are eventually acclaimed; and there are fashions in taste. In both cases good works of art suffer mainly because of non-aesthetic considerations (and second-rate works are boosted by them); they include such things as moral and religious prejudices, conventional ways of thought and the influence of particular critics.

However, we are all individuals; no one is an Aesthetically-judging Being; he is Tom, Dick or Harry, who is also a creative artist, an interpretative artist, or a member of an audience. The question which we need to ask is not 'What is beauty?' but 'How much of oneself qua unique individual does, and ought to, enter into creative work and aesthetic appreciation?'

This relation between the individual and the object of appreciation was investigated by Edward Bullough, who termed it 'Psychical Distance'. His main exposition appeared in the *British Journal of Psychology,* vol. V, 1912, under the title 'Psychical Distance as a Factor in Art and as an Aesthetic Principle'. A further paper, 'Recent Work in Experimental Aesthetics', which appeared in the *British Journal of Psychology,* vol. XII, 1921, also throws light upon the subject.

In his original paper Bullough begins with a section on the meaning of the term 'Psychical Distance'; he then considers it as a factor in art and as an aesthetic principle. My main criticism is of the first section, parts of which are, I think, modified by later sections, by Bullough's second article, and also by other people writing since 1912. Bullough himself was never dogmatic in putting forward his conception, he frequently qualifies statements about attitudes with 'at the present time——'. In spite of this he has been quoted constantly for nearly half a century without much reconsideration or even criticism.

I am indebted to Bullough for the whole basic idea of psychical distance, but as his paper is easily accessible I shall not quote him at length. (His paper, which originally appeared

in the *British Journal of Psychology*, vol V, 1912, has recently been reprinted in 'Aesthetics; Lectures and Essays', E. Bullough, ed. E. M. Wilkinson, Bowes & Bowes, London, 1957.)

The theory I put forward is in the main Bullough's modified; wherever I have disagreed with him both sides of the case are stated.

. . . . . .

Any mental state which occurs within and on the level of aesthetic consciousness may form the object of aesthetic enjoyment, whether it be an organic feeling, contemplation of a work of art, or an abstract intellectual operation. When I apprehend aesthetically I contemplate an object for its own sake, from my unique standpoint, but without consciously involving my idiosyncrasies in the relationship. This entails, I think, that the pleasure which accompanies the contemplation of a work of art ('aesthetic emotion' as it has often misleadingly been called) is of the same kind as that afforded by the *contemplation* of any job well done and no concept (such as Hegel's revelation of truth) is involved.

We often speak of 'enjoying' works of art; not only novels, landscapes and comedies, but tragedies and paintings of the Crucifixion. At first sight the word may seem a curious one to employ in such cases, and some writers have scented a basic aesthetic problem here. They have evolved 'psychological' theories about the ambivalence of human nature, or have said with Alexander 'I should gladly accept a better word if it is offered'.[1] Provided we grant the contemplative nature of aesthetic appreciation no real problem arises, I think, though a pseudo-problem may be created by the present-day usage of the word 'enjoy'. The only way in which I can help Alexander is to offer 'fruor', not 'gaudeo', as the synonym for his 'enjoy'. But it is only lately that we have come to restrict the word to its gaudeo-sense, 'to be made happy by'; the fruor-sense was common enough until well after Elizabethan times and is preserved in Biblical and legal language meaning 'to have the benefit of'. When we speak of enjoying any work of art, even tragedy, the fruor-sense is surely appropriate enough.

This process of achieving a suitably contemplative relationship has been termed 'distancing'. To explain what he means by psychical distance Bullough describes how we can enjoy a fog at sea if we abstract the danger and discomfort of the experience and consider the strange beauty of our surroundings. This

[1] Samuel Alexander, *Space, Time and Deity* (Macmillan, London, 1934), pp. 12-13.

explanation seems to me extraordinary, for surely we should not deliberately not-think about practical matters and then appreciate aesthetically? Yet this is what his somewhat vague wording suggests, at any rate sometimes (the italics are mine)—

> '. . . in the fog, the transformation by Distance is produced in the first instance by *putting* the phenomenon, so to speak, out of gear with our practical, actual self; by *allowing* it to stand outside the context of our personal needs and ends—in short by *looking at it* "objectively", as it has often been called, by *permitting* only such reactions on our part as emphasize the "objective" features of the experience, and by *interpreting* even our "subjective" affections not as modes of our being but rather as characteristics of the phenomenon.
>
> 'The working of Distance is, accordingly, not simple, but highly complex. It has a negative, inhibitory aspect—the *cutting out* of the practical side of things and of our attitude to them—and a positive side—the *elaboration* of the experience on the new basis created by the inhibitory action of Distance.'[2]
>
> 'The sudden view of things from their reverse, usually unnoticed, side, *comes upon us as a revelation*, and such revelations are precisely those of Art.'[3]

It seems to me that in this introduction to Distance Bullough's wording raises three connected questions, and fails to answer them—

(a) Do *we* put the phenomenon out of gear with our practical selves, or does *Distance* cut out the practical side of things?

(b) Do *we* elaborate the distanced experience, or does the working of Distance itself do so?

(c) Is Bullough saying that we deliberately distance experience to find beauty, but that *art* 'comes upon us as a revelation'?

Usually it is the beauty of the natural phenomenon, or the beauty revealed in the work of art by the artist who was arrested by the beauty of the phenomenon, which captures our attention, puts us out of gear with practical life, and forces us, if we are receptive, to view it on the level of aesthetic consciousness. To use Bullough's own example, but with my interpretation—I see something looming up out of the fog, I momentarily think 'How beautiful!', but almost simultaneously I recognize it to be an

[2] Bullough, *British Journal of Psychology*, V, 1912, 'Psychical Distance as a Factor in Art and as an Aesthetic Principle', p. 89.

[3] *Ibid.*, p. 90.

iceberg and raise the alarm; the aesthetic apprehension is no less real for being brief. I do not, however, think 'We are going to crash into that iceberg, isn't it beautiful'.

There is a certain timelessness about works of art and even about fleeting glimpses of natural beauty, and in them, as in dreams, concepts that reason apprehends as separate or even paradoxical are often resolved into a chord of sense. Aesthetic appreciation must be on such a plane, but to pretend to live on it is an affectation.

Many things may render us unreceptive—stupidity, sophistication, prejudice, lack of imagination, lack of experience or knowledge, or the press of personal and practical affairs. Any of these things may make us distance the object inappropriately and so prevent it from working its effect upon us, whether we are artists or spectators.

Let me give an example. We usually go to the theatre with every intention of enjoying the play. During the performance, if all goes well, we accept the whole atmosphere and time-scale of the drama, we are aware of what Charles Morgan has called 'form in suspense'. But have you noticed how the last act of a play seems to drag when you are doubtful whether you will be able to catch the last train home? Or suppose that while you are waiting for the curtain to go up on 'Othello' a picture comes into your mind of your wife hastily hiding a strange pipe when you came home the night before—will you be able to contemplate Shakespeare's play as though you had not recalled the incident? In such circumstances we cease to regard the play simply as a work of art, an object of contemplation which works its unique effect upon us. It is no longer divorced from our personal, practical life and we tend to get to work upon *it,* to distance inappropriately, to get mixed-up in the action instead of sitting back and watching its inevitable progress. We become akin to the child who shouts a warning to the film's hero and jigs up and down when the horses gallop.

If we fail to appreciate a work of art as a work of art because we become emotionally involved to an extent which precludes contemplation, we are said to be under-distanced. If, on the other hand, we are so remote from the work of art as such that we are mainly concerned with the technical details of its presentation, then we are said to be over-distanced.

Sometimes, however, we do distance deliberately and rightly so. People who have a dual relation to the phenomenon or work of art—a member of an orchestra, an actor, a translator, art critic, gardener and architect, all have a technical as well as an

appreciative and creative relation to their work. The success of each depends upon his ability to change his stance easily and quickly so that the two relations are complementary.

> 'In the carrying out of the work . . . where the communication and presentation of what is already known is the aim, the Will can and, indeed, must be again active, inasmuch as a purpose is present.'[4]

Objective apprehension is at the base of every artistic, poetic and purely philosophical concept but it cannot last long because of the press of practical affairs. We cannot long remain mere spectators in the world.

One may, however, distance the object unconsciously for reasons not contributing to aesthetic appreciation. Religious or moral beliefs or conventions may prevent one from being able to see Crucifixions or nude statues as works of art, and so on. If we have been brought up by people who are acutely embarrassed by any discussion of religion, and who regard 'nude' and 'rude' as synonymous, we may never be able to freely contemplate such works of art even though our reason has rejected the parental attitudes. To regard perfectly decent works as pornographic is, of course, an extreme but common manifestation of under-distancing. We may also consciously distance-out such prejudices when we are aware that they may make us view a work of art unjustly.

On the other hand, we may deliberately distance for illegitimate reasons; this is what Collingwood calls 'corruption of consciousness'.[5] We recognize a value, we see beauty, but for some reason we deny what we have seen. Because we are ashamed of unpopular opinions, because we are unwilling to seem to run with the herd, or are scared by a newly revealed aspect of ourselves or of our environment, we repress or misconstrue a truth that we have experienced. We may realize that a book is a great work, but because the author is someone we dislike personally, or whose politics or morals we disapprove of, some defence mechanism on the threshold between the conscious and unconscious makes us believe that it cannot really be good. Or, on the other hand, we may tell ourselves that a poor work must be good because it is held to be 'an Old Master', is the work of someone we admire, or is currently lionized. I cannot, of course, completely deceive *myself*, this would be rather like

[4] Schopenhauer, essay 'On the Metaphysics of the Beautiful and on Aesthetics' in *Selected Essays of Schopenhauer*, ed. Bax (Bell, London, 1914), pp. 278-9.

[5] R. G. Collingwood, *The Principles of Art* (Clarendon Press, Oxford, 1938), pp. 282-5.

trying to suck and blow at the same time but I can enter a guilty no-man's-land in which my true beliefs, feelings and value judgments are kept submerged.

The artist distances phenomena so that their beauty is able to penetrate barriers which would probably have come between us and the undistanced objects. Few of us would find beauty in a butchered ox, but many find Rembrandt's painting 'The Butchered Ox' beautiful. Nevertheless, the artist cannot do our distancing for us, we have to be receptive to his creation and distance it as he did the phenomenon.

> '. . . the work of art is an object which has already passed through a subject, and is hence for the mind what animal nourishment—the already assimilated vegetable—is for the body.'[6]

That is, the artist has distanced his experience, the spectator has to do the secondary distancing for himself; the sheep eat the grass, but we have to digest the mutton. The distancing process concerns and is maintained by the artist, audience and, where he intervenes, by the interpretative artist—pianist, actor, translator, and so on. Beauty can be enhanced or marred at any stage.

By translating into another medium the artist makes visible to people other than himself the beauty he has found in the object. For instance, the ox carcase that most people find repulsive is transformed into light, line and colour by Rembrandt. Now people in general may see what Rembrandt saw. But this does not guarantee that Tom, Dick and Harry will see the beauty. They have to achieve suitable distance in relation to the picture; they may see the beauty of line and colour, they may still see a repulsive carcase, a year's meat ration for ten people, or some such thing. Moreover, there is always the possibility that an artist may make the task of an individual spectator harder. X, who could have appreciated the landscape or the idea, cannot appreciate the painting or the poem. But, as Bullough says, in general the more we appreciate the one, the more we are likely to appreciate the other also.

Bullough states that

> Distance '. . . has a n e g a t i v e inhibitory aspect—the cutting out of the practical side of things and of our attitude to them—and a p o s i t i v e side—the elaboration of the experience on the new basis created by the inhibitory action of Distance'.[7]

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[6] Schopenhauer, *op. cit.*, p. 282.
[7] Bullough, *op. cit.*, p. 89.

He seems to imply here that there is a negative basis that is the 'correct' basis for aesthetic appreciation. To say this is, I think, to postulate an ideal—in the sense of unattainable—state of affairs. To be useful, the concept of distance must be stated in relative, not absolute terms. Bullough says that there is only one correct distance for each person and each thing and that this is achieved when there is

'the utmost decrease of Distance without its disappearance'.[8]

However, in the next section he modifies this rigid statement, saying that one of the advantages of 'Distance' is its variability; it contrasts favourably with 'objectivity' and 'detachment' which preclude the idea of a personal relation, are rigid and exclude their opposites.

> 'Distance, on the contrary, admits naturally of degrees and differs not only according to the nature of the object, which may impose a greater or smaller degree of Distance, but varies also according to the individual's capacity for maintaining a greater or lesser degree. And here, one may remark that not only do persons differ from each other in their habitual measure of Distance but that the same individual differs in his ability to maintain it in the face of different objects and of different arts.'[9]

Bullough admits to there being degrees of distance, I want to go further and say that there is no outright 'correct' distance, and that aesthetic appreciation itself is capable of degrees and can occur within a certain range of an optimum distance which is always relative to person, object and occasion.

I am well aware that in saying this I am running counter to most, if not all, established theories, even Bullough's. But I maintain that to stipulate that an intelligent artist or spectator who apprehends aesthetically is necessarily 'distancing correctly' (Bullough), 'apprehending entirely on the plane of illusion' (Susanne Langer), or 'being objective', is as erroneous as to assume that intelligent people always think logically and act morally. We are not Moral Beings, Rational Beings, or Aesthetically-apprehending Beings; we are human beings, and to label ourselves with abstract absolutes is simply to handicap down-to-earth criticism and constructive thinking.

According to Kant an interested (unsuitably distanced) appreciation has nothing to do with aesthetic appreciation—

[8] Bullough, *op. cit.*, p. 94.
[9] Bullough, *op. cit.*, p. 94.

unless it be admitted in adherent beauty, the beauty of a flower seen by a botanist botanizing (and this is over-distancing). If our ability to appreciate disinterestedly manifests itself we have been led forward not by a glimpse of beauty, but by a glimpse of moral good. Bullough admits that we can, according to our capacity, pass from under-distanced 'subjective' appreciation to true aesthetic appreciation. We do this, he says, because we find some measure of beauty in the object when we under-distance; this may lead us to seek what we recognize to be of greater value, beauty rather than charm.

This is, I think, a great step in the right direction, but it still assumes that there is 'correct distance' and 'true aesthetic appreciation'; if we attain these ideals we have, as it were, hit the jackpot, and universal agreement in aesthetic judgment is achieved. But before we can have 'true aesthetic appreciation' we must have an ideal negative basis for aesthetic experience; there must be

'the utmost decrease of Distance without its disappearance'.[10]

I think that in order to be realistic and have practical application the concept of distance must be stated throughout in relative not absolute terms. I should say that there is an optimum (not 'correct') distance for each person, each object and each occasion; but that aesthetic appreciation can take place other than at the optimum point, though not too far from it.

under-distanced	appropriately distanced	over-distanced
subjective 'wallowing'	aesthetic appreciation	detached, critical, technical, intellectual attitude.

The relation between the parts of the scale (save by coincidence) will vary for every person, object and occasion; unless this is so the concept cannot be useful and related to our actual experience. For instance, an adolescent, an adult spectator and a stage manager may all appreciate *Romeo and Juliet* aesthetically; yet the adolescent has not the emotional maturity to appreciate it as fully as the adults, and the stage-manager, even *qua* spectator, will probably be more aware of technicalities than the others.

If this contention is allowed to be true I can construct a sliding scale to illustrate the operation of distance in relation to a work of art—

[10] Bullough, *op. cit.*, p. 94.

Complete identification with the hero. The poem qua story ('and then' sequence). My own work, a friend's work, evoking 'it reminds me', tears, etc.	The Poem	Complete detachment. The poem qua argument ('and so' sequence). Prosody. Evidence of the poet's psychological make-up, etc.
under-distanced	appropriately distanced	over-distanced
subjective 'wallowing'	aesthetic appreciation	detached, critical, technical, intellectual attitude.

(The vertical lines and crosses are to be regarded as mutually fixed, but movable in relation to the horizontal lines. The crosses mark the most complete comprehension and appreciation of which the individual is capable. The position shown is the central one, *not* the 'ideal' one, for the question is one of relative appropriateness. The position shown would represent an unsuitable state of affairs in relation to a work that was itself highly or only slightly distanced.)

I will now reiterate what I mentioned at the beginning of this paper, that the subject-object relation in aesthetic judgment is not the same as in scientific calculation (though those who insist upon a 'correct distance', 'complete illusion', and so on necessarily imply at any rate the possibility of calculations; of there being only one right answer to any question involving taste). When I see that something is beautiful I do not stand in the same subject-object relation that I do when I see that 2×2 is 4; this formula is correct for all people and all times, anyone who denies it is wrong. Granted, I cannot say 'This is beautiful *for me*' but I may say 'This *is* beautiful' and have no one agree with me. Does this mean that either I or they are 'incorrectly distanced'? Surely not necessarily; it simply reflects the fact that we are different people. If I find someone who delights in the same things that I do, it is as though we work on the same wavelength and is a priceless relationship, whereas someone who always gets the same answers to his calculations as I do means nothing special to me because of it.

When I judge something to be beautiful I am not obtruding myself into the judgment, but I *am* appreciating the object for its own sake from my unique point of view. My judgment, even though universally valid if true, is yet the result of the particular object's reacting with my particular personality, experience, knowledge and imagination. Aesthetic appreciation though distanced and disinterested is not detached. Individual people judge

individual phenomena to be beautiful; Man does not recognize Beauty.

(Kant, who is interested in what makes men alike, not in what makes each a unique personality, speaks invariably about the reaction of men in general—

> 'The preliminary study for fine art lies, not in rules, but in culture of the mind to be got from the humanities; on the ground that humanity lies, on the one hand in the general feeling of sympathy, and on the other in the faculty of communicating our inmost thoughts and feelings. These two together constitute the social spirit whose possession distinguishes man from the animals.'[11]

After this he often speaks of 'man qua man' but never comes down to the level of existing human beings with their differing capacities for sympathy and communication. He says that men disagree about judgments of taste only because they have not 'seen' the object properly or because they misinterpret their state when confronted by it. If men see and think aright then they will make truly aesthetic judgments and come to the same universally valid conclusions. Bullough, too, implies that we disagree in our judgments of aesthetic objects because of our shortcomings. But Kant, unlike Bullough, does not allow that some men are incapable of sustained contemplation; that such people can gain some measure of aesthetic enjoyment perhaps from their under- or over-distanced apprehension; that what we find and value in an object largely depends upon what we bring to it—and this without making our judgment 'subjective'. For Kant 'men' refers necessarily to practising Rational Moral Beings, not to fallible living creatures like ourselves.)

I agree with Kant and Bullough, among others, that an aesthetic judgment is logically singular. It follows that we can make no comparative value judgments about beauty. Just as I cannot meaningfully say 'calculation A is correct and calculation B is correct, but A is more correct than B', so I cannot say 'both A and B are beautiful, but A is more beautiful than B'. (I can of course say 'A is a better drawing than B' or 'I like A more than B', just as I can say that one correct calculation is neater or more lucid than the other.)

Bullough would seem to imply that all beautiful objects are incomparable because they are on a par; they have all scored top marks. (I suspect that Beauty with a capital B lurks here.) I think that Kant's explanation of the singularity of aesthetic

[11] Kant, *Critique of Aesthetic Judgment*, appendix on 'The Methodology of Taste'; Meredith's edition, O.U.P., p. 355.

judgments is preferable. He says that I cannot isolate a concept which makes me judge one individual beautiful, therefore I cannot possibly attach the predicate to a genus, I can only judge each individual as I come across it. Each individual remains its unique self and is incomparable. Nevertheless Kant does, I think, make a blunder here. He postulates disinterested-human-being apprehending individual aesthetic objects. (Bullough was at least consistent, though I think wrong, in speaking of correctly-distanced-human-being apprehending beauty.) Surely if works of art are individuals so must be the people who create them. An individual can create an individual or a type, but a type cannot create an individual. Matter can be informed with individuality only by an individual.

The wider and deeper our experience of all kinds—*provided that we can utilize it on a contemplative level*—the greater is our appreciative and potential creative capacity likely to be. There are undoubtedly people who would like to ban 'The Merchant of Venice' because they find it anti-Semitic, 'Medea' and 'Othello' because they detect racial conflict, Praxiteles' nude figures because they feel them to be pornographic, and so on. Others of us think that they are wrong in their objections, but we can see why they do object; they are thinking on a practical level, are not looking upon the works of art as works of art; in short, they are not suitably distanced.

If a work of art is to have the greatest possible chance of being successful it must be uncomplicated, seeming-spontaneous and 'universal' (this does not mean uncomplex, facile and obvious). Bullough says that the artist

> '. . . will prove artistically most effective in the formulation of an intensely personal experience, but he can formulate it artistically only on condition of a detachment from the experience qua personal. Hence the statement of so many artists that artistic formulation was to them a kind of catharsis, a means of ridding themselves of feelings and ideas the acuteness of which they felt almost as an obsession. Hence, on the other hand, the failure of the ordinary man to convey to others at all adequately the impression of an overwhelming sorrow or joy.'
> 'What is therefore both in appreciation and production most desirable is the utmost decrease of Distance without its disappearance.'[12]

'Utmost' must be, according to my formulation, a term relative to person, object, medium and occasion. Certainly it is necessary

[12] Bullough, *op. cit.*, pp. 93-4.

that the artist should distance the experience that fires his imagination, but the extent to which this needs to be done depends upon the nature and intensity of the experience and the sophistication of the artist and his audience. A minimal distance is generally assured by the very fact that the artist translates into a new medium, that, to use Langer's term, he creates illusion. It is at any rate certain that some experience demands greater distancing by the artist than other kinds; terror more than awe, sexual passion more than affection, and so on. (Unless he is deliberately creating unsubtle horror stories or pornography, then he will decrease distance as far as his medium allows—though we should hardly call such crude productions 'art')

A simple experience that we have all enjoyed is often most effective when least distanced. A pastoral poem about sunrise, stuffed with conceits and highly distanced, has a limited appeal; it can be appreciated only by those versed in its conventions and allusions. The song 'Oh, What a Beautiful Morning!' distances what we have all felt on such a morning by putting into sound what is usually only a feeling of well-being and a stretch in front of a window. Strephon's limbering-up at sunrise for the midsummer ritual dances, accompanied only by vibrant chords like the twang of his own taut sinews was the most poetic and effective scene I have ever witnessed in the theatre. (Michael Tippett's 'Midsummer Marriage', beginning of Act 2; Pirmin Trecu as Strephon.)

In these examples it is safe to say that the most successful are the least 'detached' qua personal—but the personal experiences are common to all of us; we all enjoy a beautiful morning, and we do it spontaneously. Therefore the artist has no need to de-personalize his experience to any great extent; and if he distances for us, apart from translating into his chosen medium, the work becomes pretentious.

Bullough's statement that the artist can formulate an intensely personal experience artistically *only* on condition of a detachment from the experience qua personal must, I think, be coupled inseparably with his later statement

> 'What is most desirable both in appreciation and production is the utmost decrease of Distance without its disappearance.'

An artist's success is established by his work, not by the precepts to which he conforms.

A work of art or a dramatic character will be effective, ring true, and be credible within its context, only so long as a certain consistency is maintained. The consistency may derive from the

natural unity of the subject or from the mode of presentation; it reduces to consistency of distance on the part of the artist, interpretative artist, and audience.

If inconsistency of distance occurs in the work of the artist it must be deliberate and for a purpose—as in mock-heroic verse. During a play the distance may be skilfully altered but it must never be lost. One remembers the horrible loss of distance in 'Peter Pan'—the moment when Peter says 'Do you believe in fairies? . . . If you believe, clap your hands!', the moment when most children would like to slink out of the theatre and not a few cry—not because Tinkerbell may die, but because the magic is gone. What, after all, should we feel like if Lear were to leave Cordelia, come to the front of the stage and say, 'All the grown-ups who think that she loves me, shout "Yes" ?

One of the most persistent criticisms of Shakespeare's tragedies has always been that the 'comic' scenes are inappropriate to tragedy, and that he would have done better to leave out 'sops to the groundlings'. People who make this complaint are, in fact, largely condemning what seems to them to be inconsistency of distance. Although we may descry a serious purpose in these scenes, a forwarding, underlining or echo of the tragedy, yet it cannot be denied that they are differently distanced from the rest of the play. But Shakespeare, unlike Greek tragedians, is not writing against a cosmic background; he writes about people, and in a single play he shows life in the round. His 'comic' scenes in tragedy are, from this point of view, somewhat comparable with the Satyric play in a Greek tetralogy. But unlike them they do not lose the distance suitable to tragedy, they only vary the emotional pitch so that distance shall not be lost.

Both artist and audience must continually alter their distance, just as the eye must constantly move and focus to enable us to see properly. Eliot's 'Murder in the Cathedral' is a play which demands almost acrobatic changes of distance on the part of the audience—the sermon does demand 'the utmost decrease of distance' for the audience *is* the congregation to which Thomas is preaching; but this minimal distance is not appropriate to our relationship with Thomas and the Knights, nor even with the Chorus which is everyman. There is always a range within which consistent distances will lie. The 'Porter scene' in 'Macbeth' lies, I think, within the range; Peter Pan's appeal to the audience is way outside it.

A difficulty may seem to arise here. How can a work 'demand' changes of distance on the part of the audience if there is no 'correct' distance? If every individual's optimum distance is peculiar to him, does this not mean that anyone's judgment of a

work of art and his approach to it are as good as anyone else's? Common sense tells us that this is not so. I am not prepared to back my judgment of painting to the same extent as my judgment of poetry; some people's optimum points for appreciation of a work of art fall below the level of aesthetic appreciation altogether, their best is just not good enough; and some judgments made in good faith are simply wrong.

There is nothing suspect or esoteric about saying that something can be wrong yet never absolutely correct—I can never give an absolutely correct rendering of π, but I can certainly give a wrong one; and in the same way I can never say that I have finally and completely understood 'King Lear' or 'The Ancient Mariner', but I can certainly fail to understand and appreciate them. (I can work out π to forty decimal places: I can say that 'King Lear' and 'The Ancient Mariner' are disquisitions on the nature of love. I can stop at $\pi = 3.14$: I can say that 'King Lear' and 'The Ancient Mariner' are stories about - - - -. I can also say that π is 99, or that 'King Lear' and 'The Ancient Mariner' are Restoration comedies, and I shall be utterly wrong in both instances.) Though I can no more exhaust the possibilities of a great work of art than I can 'get' π, yet it is perfectly sensible to speak of 'the poem', 'the sonata', and so on, and this entity I have called a conceptual form.

When we appreciate a poem as a conceptual form we are contemplating the whole; all its parts are fused and our attention is not dissipated or side-tracked by them. Nevertheless it is probable, and in a sustained work I should say certain, that I shall not be sheerly contemplating throughout. I shall be aware of story, rhythm, argument, and so on, although I shall not think about them. Indeed I cannot hope finally to grasp the conceptual form of, say, 'Paradise Lost' unless I have kept in mind the story and the argument as I go along. The poem is neither a story nor an argument, but these elements do contribute to the poem, and to appreciate it aesthetically we must be aware of them, though not concentrated upon them apart from the poem. We cannot, in fact, hope to contemplate a complex work until we have put in some preliminary study and become familiar with its many facets.

(Although I insist that good works of art are conceived as wholes, as conceptual forms, I am not prepared to go the whole hog and say that a work of art is by definition a conceptual form. We might well deny 'Tamburlaine' to be a conceptual form, for it is a very patchy drama; but we should not deny it to be a work of art, we certainly value it as poetry.)

In addition to insisting that the aesthetic appreciation is possible when one is suitably, as distinct from 'correctly', dis-

tanced, I should like to make a further distinction. When an intelligent mature individual is suitably distanced he appreciates the work of art as a whole; this is, as it were, his 'best' optimum, but he is not capable of attaining it in relation to every work and all media. When he cannot grasp the conceptual form, the poem, he is not necessarily debarred from all aesthetic appreciation, he may appreciate aspects of the work. Certainly aesthetic appreciation is not confined to the appreciation of conceptual form, to pure contemplation, nor even to this plus apprehension of Kant's 'adherent beauty'. If it were then children and probably most adults would never aesthetically appreciate any work of art. It is, I think, incontestable that children do find aesthetic enjoyment in adult works of art as well as in works specially created for them whose conceptual form they might be expected to apprehend. They can appreciate the poetry of 'The Ancient Mariner' without appreciating the *poem* except as narrative.

Indeed one might say—in terms of the sliding scale (p. 164)—that a child's optimum distance lies towards the left-hand side. Probably we naturally progress from the left towards the centre, though the original and final positions and rate of progress vary for each individual. There is, I think, no such general approach from the right-hand side. An individual might, I suppose, begin by studying poetry because he was interested in speech therapy or some such thing, and later come to value it for itself, but this would be exceptional. Still the optimum point for many people will tend to the right of the scale, their aesthetic appreciation has an intellectual rather than an emotional bias.

Distancing on the part of the audience is fairly easy to understand. When we are looking at or hearing a work of art the fact that we know what we are doing normally ensures that distance does not disappear. We do not shout a warning to Gertrude when she picks up the goblet of poisoned wine (though children and unsophisticated audiences do tend to cheer the hero and boo the villain).

The position of the interpretative artist is also relatively simple. The actor 'is' his rôle as nearly as may be without losing sight of the play as a conceptual whole. The instrumentalist or singer performs with the whole work in mind, so far as his individual technical and creative preoccupation permits. The translator maintains the distance of the original as nearly as is possible in a different language.

The creative artist has more choice; at any rate there is no external *aesthetic* compulsion to dictate his distance; the work does not exist until he has created it. A work inspired by personal experience has a certain internal compulsion; the poet is

writing primarily for himself, and his choice is between creation and silence rather than between creation of a highly- or little-distanced work. Although the artist is initially free to create as he will, the distance is 'built into' the conceptual form when it is conceived. It is only when he creates for a particular audience that the artist considers distance as a separate factor while he is in the process of creation, and here the compulsion is external and not an aesthetic dictate.

> 'What is both in appreciation and production most desirable is the utmost decrease of Distance without its disappearance.'

We can now amend part of this to—What in interpretation and appreciation is most desirable is to achieve and maintain the artist's distance as nearly as possible. (I say 'achieve' because we may have to work to attain this most desirable distance; to analyze and then recreate for ourselves.)

The distance most desirable in creation is far more difficult to determine. (I shall write here only of the artist qua artist—not of the artist qua poet-laureate, writer for children, for his co-religionists and so on; for in these cases he is consciously catering for an audience with special claims and demands.) The artist must distance the experience that fires his imagination or the theme of which he treats. The nature of this initial impulse combined with the individuality of the poet dictates the distance for each piece of creative work. His task is to make sharable his personal experiences and ideas in so far as he deems them to have significance and value for other people. He must distance so that their significance for human beings in general—not for himself in particular—is revealed.

The obvious objection to make here is that there is no such thing as 'human beings in general'. The possible readers of poetry are intelligent or stupid, simple or sophisticated; and in any case most people do not read poetry of any kind once they have left school.

The poet, of course, knows this, but he is not trying to recruit poetry-readers; he is aware that most people will not read his work and that many who do will gain nothing from it. But he is writing primarily for himself; once a particular poem is written he is a member of his own audience. *If* he can view his own work disinterestedly he is as good a judge of its merits as any other individual. The poem does not exist until it has been said (aloud or internally) or written in its entirety, and if it was conceived as a conceptual whole then it is born as such. The poet as a craftsman works at and polishes the poem, and when it is ready to be published it should no longer be a piece of his

private property. The poet is not even in a privileged position with regard to his poem when he is contemplating it.

If the final product still only recalls to him the personal experience and/or the technical process of creation, then he has failed to satisfy himself, and the work invites others as well as its maker to wallow and/or criticise, not to enjoy. I think we may say that if the poet can contemplate his own poem then it is a suitably distanced work. We may say, in a Kantian way, that the poet's judgment of his own work is subjective yet universally valid.

I would amend Bullough's assertion thus—

What is most desirable is in production to create a conceptual form; in interpretation to present this conceptual form; and in appreciation to contemplate it.

Conceptual form is discovered by the artist who contemplates his experience and ideas. He embodies it in his work, then any reader, himself included, may rediscover it.

> 'The poet's signature lies in the individuality of the poem not in the novelty of the symbols. Only when a thing signs to a poet does it become part of his symbolic vocabulary. Only assimilated material can be embodied by the poet. Given these things then the poem will have and retain the power to re-awaken an imaginative experience.'[13]

If we share the poet's distancing capacity, if we 'work on the same wavelength', the conceptual form, 'the poem', may flash across to us; otherwise we must work to achieve it, analyze and then try to synthesize his creation. If we are capable of attaining a distance approximate to the artist's own, if our optimum distance is akin to his, then we may do that most desirable thing—enjoy the work as a conceptual form.

To use an analogy—the aim of the spectator must be to see a painting as the artist qua spectator sees it. To do this the short-sighted will come close, the long-sighted will stand back; the blind cannot succeed by any manoeuvre and will probably not bother to try. But for anyone who is not totally blind there is an optimum distance from which he can get his best possible view of the work. It may be that even at his optimum an individual cannot see the work as a whole, he may be able to enjoy only the colouring, or the craftsmanship which has produced some detail, but even this limited disinterested appreciation is for him aesthetic appreciation. It may even be that an artist has peculiar eyesight, and in this case most people will find his work odd and obscure or out of focus; but if the artist can see a conceptual

[13] Kathleen Raine, 'Collected Poems', Hamish Hamilton, London, 1956, Preface.

form there from his optimum distance, then it is probable that the spectacles of familiarity, study and the passage of time will enable many people to share his view.

To summarize the points made in this account of distancing—

1. Any mental state from the obscurest organic sensation to the most abstract intellectual operation may form the object of aesthetic appreciation, provided that it occurs within and on the level of the aesthetic consciousness.
2. It occurs on this level when we, to use Alexander's terminology, 'live through' rather than 'witness' it; when we enjoy it (fruor not gaudeo); when it is suitably distanced.
3. Usually it is the beauty of the natural phenomenon, or the beauty revealed in the work of art, which captures our attention and forces us, if we are receptive, to view it on the level of aesthetic consciousness.
4. Prejudice, stupidity, adherence to convention or habitual rejection of it, lack of imagination, the press of personal affairs, and so on, may render us unreceptive; that is, may make us distance the object inappropriately and so prevent its beauty from acting upon us.
5. People who have a dual relation to the work of art or natural object do distance deliberately—critics, interpretative artists, scientists who deal with beautiful objects, and so on.
6. Often the artist distances the phenomenon so that its beauty is able to penetrate barriers which would have come between us and the undistanced phenomenon.
7. Distance is susceptible of degrees, the optimum distance for appreciating anything aesthetically varies with the person, medium, object and occasion.
8. Aesthetic appreciation itself is susceptible of degrees and can occur within a certain range.
9. An individual's capacity for aesthetic appreciation is usually greater in respect of some media than others.
10. When I judge something to be beautiful I am not obtruding myself into the judgment, but I am appreciating from my own unique distance. My judgment (even though it be universally valid if true) is yet the result of the object's reacting with my personality, experience, knowledge and imagination.
11. Aesthetic appreciation though distanced and disinterested is not detached. That is, no matter how many people's judgments may agree they are still individuals judging objects beautiful, not Man (nor even men) recognizing Beauty.

12. The wider and deeper our experience of all kinds—provided that we can utilize it on a contemplative plane—the greater is our appreciative and potential creative capacity likely to be.

    (One has to admit here though that it is sometimes the very limitation of experience which allows an artist to contemplate life. This applies especially, perhaps, to women novelists in the past. Would the work of the Brontës have benefited if they had seen less of each other and more of the world? What would Jane Austen have written if her 'ignorance that was almost innocence' had been remedied?)

13. If a work of art is to be simple, to give the impression of spontaneity, and to have universal significance, then what is most desirable is in production to create a conceptual form; in interpretation to present this conceptual form; and in appreciation to contemplate it.

14. A work of art qua work of art holds our attention, succeeds in sustaining its mood, and is 'true', only so long as a certain consistency of distance is maintained; not a 'correct' distance, but an appropriate range of distance.

Granted my divergences from Bullough's account of Psychical Distance, I agree with him that distance is one of the distinguishing features of the aesthetic consciousness,

> '. . . of that special mentality or outlook upon experience and life, which . . . leads in its most pregnant and most fully developed form, both appreciatively and productively, to art'[14]

and distancing seems to me to be the best account of artistic truth and of our varying ability to recognize it.

I conclude that distancing is essential to aesthetic appreciation of any kind and is an essential prerequisite and concomitant to the creation, interpretation and appreciation of any work of art; that it is, in fact, a universal aesthetic principle.

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[14] Bullough, *op. cit.*, p. 118.

CRITICAL NOTICE

WORD AND OBJECT. By Willard Van Orman Quine. New York and London, The Technology Press of Massachusetts Institute of Technology and John Wiley and Sons, Inc., 1960, xv, 294 pp. $5.50.

(1) The courses in the philosophy of language which Professor Quine gave at Harvard after the war, and his lectures at Oxford and London, were stages in the development of this book, which is one of a series of studies in communication. Parts of it were presented as the Gavin David Young Lectures in Philosophy at the University of Adelaide in June, 1959, and parts as lectures at the University of Tokyo later in the same year. Those who are already acquainted with the contents of this book will welcome the opportunity to give it the careful study that is demanded by prose which, although (often) elegant and amusing, is sometimes condensed to the point of enigma. And those who are already acquainted with the work of Professor Quine will welcome a book in which theses for which he is already well known are developed and related. But as well as criticising modal logic and the notion of analyticity, discussing referential opacity and arguing for an extensional logic, Quine presents a new thesis which he calls the principle of indeterminacy of translation. It is this principle, together with Quine's views on ontology, that gives unity to *Word and Object*, and makes it not just of technical interest as studies in semantics and logic, but of more general interest as the statement of a philosophical position.

In this notice we will concern ourselves primarily with Quine's new thesis, with his views on ontology, and with the way in which all this is related to his views about synonymy and analyticity. For convenience we will follow Quine in using "ontic" to mean "as to what there is". A discussion of those parts of *Word and Object* which we mention in (6) but do not discuss, will be found in Mr. Geach's review of this book.[1] Obviously we do not accept the view that these are among the most important parts of *Word and Object*. Because we have already met incompatible interpretations of parts of this text, as much as half of this notice (Sections 2 to 7, and 11) is given almost entirely to exposition, the remainder (8 to 10) being mainly critical.

Although we do not accept some of the theses put forward in *Word and Object*, we have no hesitation in saying that the

[1] *Philosophical Books*, vol. II, No. 1, January, 1961, p. 14 *et seq.*

scope of this book, the boldness of its main contentions, and the ingenuity of its arguments, combine to make it exciting and important.

(2) According to Quine, anyone who says, for example, "There are classes which have classes as their members", is thereby committed to the existence of classes. Of the distinction between there being two senses of "there are" and there being only one, he can make no sense, but even if "there are" is ambiguous, the existential quantifier is not, for it is by stipulation a means of stating one's ontic beliefs. He who declines to explain his view in terms of quantification thereby refuses to say what he takes himself to be talking about.

Ontology is a part of science distinguished only by the breadth of its categories. Given that there are physical objects, it is the natural scientist who decides whether there are wombats or not, and given classes, it is the mathematician who discovers whether there are even prime numbers or not, but whether there are physical objects and classes or not is the concern of the philosopher. In both aim and method the philosopher is a scientist, for in attempting to formulate the simplest conceptual scheme adequate for the suitably regimented sciences he is concerned, in the most general way, with what the world is like, and his theories must, in the most indirect way, save the appearances. In order to find common ground, the parties to an ontic dispute may have to discuss the use of words: two philosophers may, for example, talk about the use of certain words rather than about the existence of attributes, and this gives philosophy a linguistic appearance. But this appearance is deceptive, for a shift to a meta-language is possible in any theory, and it is made necessary in philosophy only by extreme generality.

Progress in the sciences is directed towards but not dependent upon the integration of established theories. Such integration may lead to any one of many equally satisfactory accounts of the world, each with its ontic theory, and there is no sense in asking which of these accounts is true, for "true" can be applied only within a theory.

(3) The second chapter of *Word and Object* is a discussion of translation, one of the aims of which is to show "how much of language can be made sense of in terms of its stimulus conditions, and what scope this leaves for empirically unconditioned variation in one's conceptual scheme" (p. 26). If we were to find a hitherto isolated tribe with whom we had nothing linguistic or cultural in common, the translation of their language would be peculiarly difficult. In fact, radical translation, as Quine calls

it, never has to be undertaken, but actual translation has been sufficiently like it often enough for linguists to have considered the matter. Radical translation must begin, not with words, but with those sentences that have a comparatively direct relation to stimulus conditions, and, in order to investigate rather than just listen, the linguist must frame and check hypotheses about native assent and dissent.

Quine defines the stimulus meaning of a sentence for a person in terms of the class which has as its members the kinds of stimulation which would prompt the person's assent to the sentence. In one place the stimulus meaning of a certain kind of sentence is said to be "the native's total battery of present dispositions to be prompted to assent . . . to the sentence" (p. 39). Since, for Quine, a stimulation is an event that produces changes in afferent nerves, while a disposition of the kind in question is a comparatively permanent neural condition, this one remark is incompatible with the repeated references to stimulus meanings as classes of kinds of stimulations, and must be discarded in favour of them.

Occasion sentences (e.g. "John has hiccups") are distinguished from standing sentences (e.g. "salt is soluble in water"): "standing sentences contrast with occasion sentences in that the subject may repeat his old assent or dissent unprompted by current stimulation when we ask him again on later occasions, whereas an occasion sentence commands assent or dissent only as prompted all over again by current stimulation" (p. 36). Observation sentences are occasion sentences the stimulus meanings of which are not changed by collateral information. The kinds of stimulation which prompt a native, or a child, to say "Car" are increased when he learns that cars throw up dust on gravel roads. The stimulus meaning of "It's blue" is sufficiently immune from the effect of collateral information for the sentence to be called an observation sentence, but the stimulus meaning still changes for the man who learns that blue things look green in yellow light. Even closer to the complete immunity of the ideal observation sentence are such sentences as "It hurts".

Quine is now in a position to define intrasubjective stimulus synonymy as sameness of stimulus meaning for one speaker. Two sentences are socially stimulus synonymous if they are intrasubjectively stimulus synonymous for nearly everyone who speaks the language. He also defines a sentence as "stimulus analytic for a subject if he would assent to it, or nothing, after every stimulation . . ." (p. 55).

Using the methods so far described, the linguist is able to translate observation sentences, although subsequent native

behaviour may of course lead him to amend his translations. He is also able to translate truth functional connectives, for a conjunction, for example, may be recognised as a compound of sentences to which the native is prepared to assent when and only when he is prepared to assent to each component. Stimulus analytic and stimulus contradictory sentences can be recognised, and occasion sentences can be found to be intrasubjectively stimulus synonymous, although they cannot be translated unless they are observational. In order to complete the radical translation of a language, the linguist, in addition to using these methods, must frame analytical hypotheses. "He segments heard utterances into conveniently short recurrent parts, and thus compiles a list of native 'words'. Various of these he hypothetically equates to English words and phrases . . ." (p. 68). What the linguist can observe cannot take him beyond results of the four kinds listed above, and consequently conformity to these results is the only empirical requirement that can be made of a set of analytical hypotheses.

It is Quine's contention that in the radical translation of any language there will be many sets of analytical hypotheses that fit all native dispositions to speech behaviour, and yet lead to incompatible translations of the countless non-observational sentences in the language. Suppose that, observing the circumstances in which a native says "Gavagai", we write down "Rabbit" as a tentative translation. Confirmation of the stimulus synonymy of these occasion sentences does not establish that the terms "gavagai" and "rabbit" are synonymous or even co-extensive. Whether "gavagai" is to be taken to apply to rabbits, temporal stages of rabbits, integral parts of rabbits, or something even less obvious to us, can be settled only when we can ask questions like "Is this the same rabbit as that?". Now this cannot be done until we have translated articles, pronouns, the identity predicate, and any other parts of speech that make up the native system of reference. And since translating these is part of what we do when we adopt a set of analytical hypotheses, there is more than one way of doing it. The question that we translated as "Is this (the same) (rabbit) as that?" might, on another set of analytical hypotheses, be translated as "Is this (a rabbit stage) (of the same series) as that?", and both sets of hypotheses could fit the facts, for the change in the translation of "gavagai" is compensated for by the change in the translation of the identity expression. If the first set of analytical hypotheses is adopted, answers to our question, and others like it, will confirm that "gavagai" is used to refer to rabbits, but if the second set is adopted, the same answers to the same questions will confirm that "gavagai" is used to refer

to "rabbit stages". Analytical hypotheses are to a certain extent wrongly so called, for although they are susceptible of empirical test in the ways already noted, there is no sense in our asking which one of a number of empirically satisfactory sets of analytical hypotheses we would be right in adopting for there is nothing here for us to be right or wrong about. To sum up, the principle of indeterminacy of translation may be stated as follows: "manuals for translating one language into another can be set up in divergent ways, all compatible with the totality of speech dispositions, yet incompatible with one another" (p. 27) and there is no sense in asking which is the right manual (see p. 73).

This discussion of a kind of translation that is never in fact undertaken is meant to reveal a feature of language which may be described more briefly. Suppose that we write down sentences of a certain language, say English, and next to each sentence we write the same or another English sentence. In this way we begin the incompletable task of mapping the language on to itself. Quine's thesis is that it is theoretically possible to do this in such a way that countless sentences have as their correlates sentences which are not in any plausible sense their equivalents, and yet if a speaker were to use the correlate of each sentence instead of the sentence itself, the ways in which he is disposed to react verbally to non-verbal stimulation would be completely unchanged. Analogous to this, but lacking clear empirical sense, is the contention that two men could be alike in all their dispositions to verbal behaviour, and yet differ in that, related to any of countless sentences, the two men have in their minds qualitatively different ideas. Quine argues for indeterminacy by clearing up alleged confusions, such as a belief in the privileged position of the bilingual, that might prevent one from seeing that his thesis is a consequence of the fact that "sentences translatable outright, translatable by independent evidence of stimulatory occasions, are sparse and must woefully underdetermine the analytical hypotheses on which the translation of all further sentences depends" (p. 72).

(4) Of two compatriot linguists working independently on the radical translation of a language, one of them might conclude that he and the native see the world in much the same way, as consisting of tables, chairs, huts and rabbits, while the other finds that the native speaks of rabbit-stages, and concludes that the native outlook is very different from his own. It follows from the Principle of Indeterminacy of Translation that there is no empirical way of deciding whether the native sees the world in one way rather than in the other. "I am not sure", says Quine, "that it even makes sense to ask" about this, and later he describes

cultural contrasts that arise from differences in analytic hypotheses as beginning to be "threatened with meaninglessness" (p. 77). Again the point may be made without talking of radical translation: is one of the linguists to suppose that the other sees the world as he does, or is he, by mapping his own language onto itself, to translate his compatriot's remarks, in order to discover an outlook that may differ from his own? It appears that Quine, having argued that no conceivable test could establish a cultural difference of this sort, is inclined to go on to say that it is meaningless to talk of such contrasts, but that he does not argue this further point, since, for him, nothing turns on it.

But not all cultural differences can be dealt with in this way. Men can distinguish a very large number of colours, and yet most languages have only about ten basic colour expressions. Thus, in any such language, short occasion sentences will be assented to upon the presentation of some colours, while much larger occasion sentences will be associated with others. A colour that is in the former position in one language may be in the latter in another. There might be a language in which there is a short occasion sentence to which a native assents on presentation of his half-brother or a pelican. Our translation of this sentence is the comparatively long "Half-brother or totem-associate", while our short "Half-brother" is the translation of a very long native occasion sentence. "Such contrasts, between peoples' . . . short-sentence partitionings of stimulations, are", according to Quine, "genuine cultural contrasts objectively describable by reference to stimulus meanings" (p. 77).

(5) It has been said that an explanation of analyticity and synonymy that would satisfy Quine would be hard to come by. In *Word and Object*, Quine claims to ask for no more than "a rough characterisation (of these concepts) in terms of dispositions to verbal behaviour" (p. 207), and much of his discussion of translation is meant to bear on the question of the possibility of such a characterisation. Taking his opponents' examples as partially clarifying these and related concepts, he compares them with his own concepts of stimulus analyticity, stimulus synonymy and stimulus meaning.

Applied to occasion sentences, intrasubjective stimulus synonymy approximates to sameness of meaning. But standing sentences are related to experience indirectly, and the kinds of stimulation that would prompt assent to such a sentence vary from one speaker to another. Thus the stimulus meaning of a standing sentence falls short of our intuitive notion of meaning, and stimulus synonymy is correspondingly inadequate. Since "socially stimulus analytic" applies to sentences to which nearly

every speaker of the language would assent after every stimulation if he would assent to anything at all, it applies not only to sentences that would normally be described as analytic, but also to these that would normally be said to convey information common to the whole community. Quine suspects "that no systematic experimental sense is to be made of a distinction between usage due to meaning and usage due to generally shared collateral information" (p. 43). This being so, stimulus synonymy departs even more widely from our intuitive notion of synonymy when it is applied to terms, for here its application is dependent upon stimulus analyticity.

Quine argues that the definition of synonymy formulated by Grice and Strawson amounts to no more than stimulus synonymy. He also maintains that in arguing that we cannot give up sentence synonymy as senseless without also giving up sentence significance, they commit the subtractive fallacy. Even if their argument were sound, it would be ineffective against Quine, who accepts that sentences cannot be "thought of as conveying meanings severally" (p. 79). A mistaken belief in sentence-significance is one of the factors that has prevented people from seeing the indeterminacy of translation, and, in arguing for this thesis, Quine is again accepting what Grice and Strawson take to be the unacceptable consequences of his rejection of synonymy.

Quine has not proved that the distinction between analytic and synthetic statements that some philosophers have wanted to make cannot be made, but, in addition to arguing that no satisfactory account of this distinction has in fact been given, he has now stated that he would accept an account in terms of dispositions to verbal behaviour. He has also shown that a great deal of work on his part has led only to analogous concepts, and he suspects that it is not possible to do better.

(6) In Chapter Three, Quine, acknowledging the influence of the psychologist B. F Skinner, discusses how children learn to use the referential apparatus of the English language, and thus master a conceptual scheme. Such deficiencies in this apparatus as vagueness, ambiguity and referential opacity are discussed, and moves that overcome these hindrances to communication are described (Ch. IV). These moves are the beginning of a regimentation of language (Ch. V) which leads to the simplification and clarification of our conceptual scheme. Ordinary language is such that a logic of it would be cumbersome and difficult to formulate. Consequently, instead of theorising in ordinary language, it is more economical to translate into, and work in terms of, a language which is ordinary except in its logical parts, which have been modified to facilitate deduction. How the former is to be

done without synonymy will be discussed in (8). If there are fewer kinds of construction and less obscurity in this language, then in moving into it we simplify and clarify our conceptual scheme, and if, but not only if, the language is, as Quine suggests it should be, one in which the only formal devices are predication, quantification and truth functional composition, then in putting a theory into it, we reveal the ontic import of that theory. Since the regimented language and the relevant parts of ordinary language differ only in their formal parts, and since the logical notation of a regimented language is introduced by means of ordinary language, in moving from the latter to the former we are in effect translating ordinary ordinary language expressions into unusually involved ordinary language expressions for which we have introduced abbreviations. These manoeuvres, of which the replacement of names by descriptions is an example, are of great technical interest, but here we can concern ourselves only with the more general features of *Word and Object*.

(7) The ontic import of a theory is revealed by a suitable notation but not determined by it. Commitments as to what there is are incurred when certain words are used in certain ways. We are committed to the existence of physical objects because of the way in which physical object terms "interplay with the whole distinctively objectificatory apparatus of our language: articles and pronouns and idioms of identity, plurality and predication . . ." (p. 236). Even though we do things for the sake of others, we are not committed to such objects as sakes, just because "sake" functions in our language only in very few of the ways in which a term does. When constructing theories we are, within limits, free to decide what expressions shall be granted termhood, and in this way we might, for example, commit ourselves to the existence of atoms and molecules but not to that of grams and centimetres. In Chapter VII, Quine discusses the sort of considerations that influence ontic decisions. We accept the reality of physical objects more readily than we do that of atoms, because typical sentences about the former are more closely associated with sensory stimulation than are typical sentences about the latter. It might be argued that by this criterion sense-data are even more acceptable than physical objects, but this advantage is counteracted by the fact that sense-data are less satisfactory from a theoretical point of view. It is maintained that "sense-data neither suffice to the exclusion of physical objects nor are needed in addition" (p. 236). On such grounds of utility for theory, classes, although not perfect, are preferred to attributes, and sentences are retained, while propositions are rejected together with sentence significance and sentence synonymy. Quine's

position, which, as he admits, is only sketched, is physicalist as opposed to phenomenalist, and extensional as opposed to intensional (Ch. VI). Since, in this context, he uses the kinds of argument that are used by many philosophers, one may ask for his grounds for saying that ontology is a part of science. They are the beliefs, mentioned in (2), that the ontologist, in working out what there is, has to take at least as much account of scientific theories as of common sense beliefs, and that he has no methods that are not used in some branches of the sciences. Ontology is only a degree wider in scope and a step further from observation than the most general and abstract parts of physics.

(8) We have already noted (6) the need to reconcile Quine's views on synonymy with his plan to facilitate theorising by replacing ordinary language sentences by those of a regimented language. According to him, to replace the ordinary language sentence S by the regimented sentence S′ is not to claim that the two are synonymous. Indeed we may be making the replacement just because S is ambiguous and S′ is not. "The real point is simply that the speaker (i.e. the person who replaces S by S′) is the one to judge whether the substitution of S′ for S in the present context will forward his present or evolving program to his satisfaction" (p. 160). Quine goes on to say that, among paraphrases of this kind, there are some that are satisfactory for the majority of plausible purposes, and that in these cases S and S′ may be described as synonymous, provided that this description "is recognised as a vague one and a matter of degree" (p. 161). S′ may be expanded mechanically into an unusually involved ordinary language sentence S″ (6) and "S′ might . . . naturally enough be spoken of as synonymous with the sentence S″" (p. 160). Understanding Quine is made difficult by the fact that in addition to saying that some pairs of sentences may be said to be synonymous, and that no two sentences are absolutely synonymous (p. 161), he also says: "Synonymy, for sentences generally, is not a notion that we can readily make adequate sense of" (p. 159). His view seems to be that we have a working notion of synonymy which enables us to speak of expressions as being more or less synonymous, or in some cases, more loosely, just as being synonymous, but that no satisfactory account of this notion has ever been given.

On Quine's account of paraphrase, any sentence may be a paraphrase of any other, since he who replaces the one by the other may wrongly judge that the replacement will satisfactorily forward his programme. If we say S′ is a paraphrase of S if and only if the substitution of S′ for S in a certain context does or would forward a certain programme, it is still the case that for

any two sentences S and S', there is a programme with reference to which S' is a paraphrase of S. To say that one sentence is a paraphrase of another is not to say much until contexts and programmes are specified. If we are teaching zoology, "A cow is a mammal" might be better as an early example than "A whale is a mammal"; if we plan to scare somebody we might do better with "There's a snake under the table" than with "There's a mouse under the table", and "John is a man who never married" could be better than "John is a bachelor" if our programme was the putting of prose into verse. It is of course clear that in this context Quine is not interested in the linguistic moves that we might make in order to amuse, frighten, or instruct people. "Among the useful steps of paraphrase", he writes, "there are some . . . that prove pretty regularly to work out all right, whatever plausible purposes the inquiry at hand may have" (p. 161). In these cases, we are told, one may speak non-technically and vaguely of comparative synonymy. It is reasonable to assume, first, that anyone who has got this far in *Word and Object* will understand what sort of purposes Quine has in mind here, although he does not give a detailed account of them, and secondly that Quine could characterise these plausible purposes if only by describing those purposes and contexts which, like the ones mentioned above, he would wish to exclude.

We will now outline a way in which some of the concepts that Quine rejects may be introduced by means of notions that he accepts. Let us note first that although the paraphrase of an ordinary language sentence may be a sentence in a regimented language, it need not be. Paraphrase is a way of avoiding trouble within ordinary language as well as a way of avoiding ordinary language. Using the notion of "forwarding a programme", we will say that sentences S_2 and S_3 are interchangeable with reference to a programme p and in a context c, if and only if the following conditions are satisfied: there is a sentence S_1, such that the replacement of S_1 by S_2 in c forwards p; the replacement of S_2 by S_3 in c does not forward p, and the replacement of S_3 by S_2 in c does not forward p. We now say that two sentences are synonymous if and only if they are interchangeable in any but certain specified kinds of context and with reference to any but certain specified kinds of programme. Here we assume, first, that from our working notion of synonymy, we know roughly the sort of programmes (e.g. versifying, summarising) and the sort of contexts, that would have to be excluded, although we do not at present give a precise description of them, and secondly that such descriptions might be worked out. It may be the case that we could never be sure that this second task had been

completed, but this is due not to any peculiarities of the concept of synonymy, but to the nature of ordinary language.

Our contention is that this notion of sentence synonymy is no less satisfactory, and no more difficult to make adequate sense of, than the notion of paraphrase without which Quine cannot talk of putting theories into a regimented language. Furthermore, analyticity and term synonymy with reference to a given set of analytical hypotheses may be defined in terms of sentence synonymy and Quine's notion of logical truth.

(9) In the last section, analyticity and synonymy were described as "some of the concepts that Quine rejects". This must now be qualified. Having listed the idioms and constructions that are to disappear when ordinary language is regimented, Quine goes on to say "that the idioms thus renounced are (not) supposed to be unneeded in the market-place or in the laboratory" (p. 228). In other places he admits that intentional idioms are not practically dispensable (p. 221), and he draws attention to the way in which he persists in using the word "meaning" long after he has disposed of the concept (p. 210). "What is involved here", he says, "is simply a grading of austerity" (p. 210). When a theory is established, it is to our advantage, from a practical and a theoretical point of view, to tidy it up by working out a rigorous and economical statement of it. In doing this, we protect the explanatory force of the theory by avoiding, as far as we can, any concepts that have defied explication. If, for example, we are unable to say what a proposition is, then we try to manage without this notion, just because there will be a consequent obscurity in any theory in which it is used. This is not to say that such notions are not acceptable in more casual contexts.

If, in a regimented theory, propositions are not admitted as values of variables of quantification, then, even though in working out the theory we may have talked of propositions, this notion is not needed in the theory, and in accepting the theory we are not committed to the existence of propositions. Nor are we committed by heuristic devices: in teaching the theory of the structure of the atom, we may begin by describing electrons as particles in orbit about atomic nuclei, although according to the theory which we accept and are teaching, there are no such particles. We have already noted (2) the contention that progress in the sciences is directed towards the integration of established theories, and here we may add that even if recalcitrant idioms can be excluded from particular theories, it does not follow that they can be excluded from "the fundamental conceptual scheme of science and commonsense" (p. 276) with which, according to Quine, the philosopher is ideally concerned. It is perhaps obvious

that any idiom, construction or concept that is indispensable in some context or other, will have to appear in any over-all picture of things, if only as an unsolved problem.

We will not at present argue for the indispensability of synonymy, but we will contend that this notion is not as readily disposed of as Quine suggests. Putting forward arguments, often in ordinary language, is a fairly common human activity, and arguments are frequently judged to be valid or invalid. This activity and these judgments will have to be taken account of by anyone who is concerned with "the fundamental conceptual scheme of science and commonsense" (p. 276). Now it is possible to show that an argument is invalid by re-interpreting the terms in it so that the premises come out true and the conclusion false, but mechanical tests are preferable when available, and methods other than re-interpretation are needed to show that an argument is valid. Often, when we wish to assess an argument, we use methods described in such books as Quine's *Methods of Logic,* and the first step is the translation into the language of truth-functions and quantifiers of the sentences of which the argument in question consists. Here we cannot discuss the adequacy of various logical systems, nor will we argue that an exact translation from ordinary to regimented language is always possible, but we do contend that what is to be aimed at in this context is synonymy, and that the type of paraphrase described in (8) will not do. The reason for this is that if, in assessing an argument, we have to replace the sentences in it in a way that will, as far as we can judge, forward the programme of the man who advanced the argument, then on the plausible assumption that he intended to argue validly, any paraphrase that makes his argument invalid is to be rejected, and consequently we can never, in his absence, conclude that his argument was invalid. What we actually do is replace each sentence in the argument by a regimented one that is synonymous with it or very nearly so, and then, if possible, test the argument. If, in the presence of the person who advanced the argument, we conclude that it is invalid, he may agree and admit that he made a mistake, or he may disagree. In the latter case, he may contend that we were wrong in replacing the sentence S_1 of his argument by S_2, and that we should have replaced it by S_3, in which case the argument would have turned out to be valid. The point now is that we can and do distinguish between the case in which we come to accept that, in the relevant context, S_1 is synonymous with S_3, and the case in which, because S_1 and S_3 are not synonymous, to replace one by the other is to change the argument. In the first case we would say, after he had drawn our attention to the context, that the argument was valid after all,

while in the second we would say that in replacing S_1 by S_3 he replaced an invalid argument by a valid one. We grant that ordinary English might be very different, and that the methods of logic might be other than those described in *Methods of Logic,* but, things being as they are, our contention is that in order to give an account of the way in which we talk about the validity and invalidity of arguments, we need the notion of sentence synonymy.

(10) In opposition to what we have just said, Quine maintains that "we cannot paraphrase our opponent's sentences . . . for him and convict him of the consequences" (p. 242). Thus if we say that any part of *Word and Object* is inconsistent, we show only that we have misinterpreted it. We will content ourselves with the observation that certain parts of this book appear to require interpretation, and that we have failed to find a consistent one.

Quine talks of the possibility of empirically unconditioned variations in conceptual schemes, and claims that indeterminacy of translation reveals the extent of such possible variations. Let us call whatever it is that might vary here, s-beliefs. In the passages quoted at the beginning of (4), and elsewhere, Quine appears to be saying that the natives have s-beliefs and so do we, but, although we can describe our own s-beliefs, we cannot sensibly ask whether the s-beliefs of the natives are the same as ours or not. This interpretation is supported by the remark that "The indeterminacy of translation invests even the question what objects to construe a term as true of. Studies of the semantics of reference consequently turn out to make sense only when directed upon substantially our language, from within" (p. ix).

We cannot ask whether the natives have the same s-beliefs as we have because, if two linguists, having adopted different sets of analytical hypotheses, disagreed about this, there could be no conceivable way of showing that one of them was right, or that one of them was wrong. But if, ignoring difficulties about private languages, we follow Quine in saying that in talking to a compatriot, we do, but might not, use homophonic translation—this being a degenerate case of translation in which each sentence is replaced by itself—then we may argue as follows. Let us divide English-speaking people into two groups. Suppose that, by the sort of mapping described at the end of (3), a member of Group One constructs two empirically satisfactory manuals for translating the utterances of members of Group Two. He finds that, according to one manual, the s-beliefs of members of Group Two are the same as those of members of Group One, but that, according to the other, the two groups have different s-beliefs. Since each manual consists of a set of empirically satisfactory analytical

hypotheses, there is no conceivable way of showing that the two groups have the same s-beliefs or that they have different ones. Now we might talk in the same way of dividing into two groups any people who have a language in common, and, for all we have said or need say, any two persons who speak the same language could be in the same group or in different ones. Consequently we conclude that it does not make sense to ask of any two persons whether their s-beliefs are the same or different. It is now far from clear that it makes sense to talk of s-beliefs at all.

When dealing with natives, we have to translate in order to communicate with them. But according to Quine, we have to translate when dealing with compatriots too, although in this case translation is usually homophonic. One way of translating the native language might be more convenient than other empirically satisfactory ways, but there would be no sense in asking which was the right way to translate, and similarly, although homophonic translation is the most convenient way of dealing with the utterances of compatriots, there is no sense in asking if it is the right way. Translating the remarks of compatriots other than homophonically would be pointless, but looking for another way of translating a native language when we already have a satisfactory way of doing it would also be pointless. It might be objected that this talk of translation as between compatriots is confused, and with this we would agree. At present we wish to argue only that if it is maintained that translation is always necessary, although sometimes homophonic, then there are no studies that "make sense only when directed upon substantially our language from within" (p. ix) although Quine claims that all but the first two chapters of *Word and Object* are devoted to such studies.

If we give up the notion of homophonic or any other sort of intra-linguistic translation, and accept that because of the indeterminacy of translation some studies do make sense only when directed upon a language from within, we may argue as follows. If L is the language of a tribe with whom we have nothing linguistic or cultural in common, there are things that can be said in English about English, that cannot significantly be said in English about L. Therefore, even if we cannot significantly speak in English of sentences in L as being or not being analytic, it does not follow from this that we cannot significantly describe English sentences in this way. In other words, even if, in dealing with a radically foreign language, we cannot get beyond stimulus synonymy and stimulus analyticity, it does not follow that we are similarly restricted in dealing with our own language.

Some of Quine's remarks are incompatible with the contention that we cannot significantly compare native s-beliefs with our

own, and thus with the principle of indeterminacy of translation. In connection with analytical hypotheses, he writes: "the more absurd or exotic the beliefs imputed to a people, the more suspicious we are entitled to be of the translations; the myth of the prelogical people marks only the extreme" (p. 69). But this is to say that it is unlikely that native s-beliefs are of a kind that we describe as absurd or exotic, and that because of this it is unlikely that any of certain sets of analytical hypotheses is the right one. If the indeterminacy thesis is to be retained, then it must be admitted that, although some native beliefs may be absurd or exotic, their s-beliefs cannot be described in this or in any other way. Consequently, as Quine elsewhere implies, our justification for the rejection of a set of analytical hypotheses that represents the native as regularly assenting to statements of the form "p and not-p" cannot be that we would otherwise impute to the native the most absurd or exotic of s-beliefs.

If native s-beliefs cannot be described even as being the same as or different from our own, then it is hard to see what we are doing when we describe our own s-beliefs, since we cannot be contrasting them with s-beliefs that are different from them. Perhaps we are contrasting our s-beliefs (B1) with s-beliefs that we might have had (B2). If this is what we are doing, we must be able to say how we know that our s-beliefs are B1 and not B2. But if we can do this, then we can describe what it would be like to discover that a native people had s-beliefs different from ours. More picturesquely, suppose we found on another planet a people who were like us even to the finest points of written language. If we have any reasons for saying that our own s-beliefs are B1, then we would have the same reasons for saying that their beliefs are B1. Thus if native s-beliefs cannot be described, our own cannot be described either, and once again we conclude that it is not easy to see that it makes sense to talk of s-beliefs at all.

It is our opinion that Quine is correct in maintaining that radical translation is systematically indeterminate, but that he is wrong in suggesting that because of this it is in principle impossible for us to know things that a native can know about the common native outlook.

(11) Those differences that Quine describes as genuine cultural contrasts (4) remain, and consequently the above objections do not rule out the possibility of different peoples having different "fundamental conceptual schemes of science and common sense" (p. 276). From the contention that we are "able to speak of the truth of a sentence only within a more inclusive theory" (p. 75), it follows that we cannot speak of the truth of a fundamental

conceptual scheme, and this Quine accepts. If studies of the semantics of reference are necessarily parochial, then we can never say of two fundamental conceptual schemes that they are qualitatively the same, although we can say that they are different, but if the objections in (10) can be sustained, then it is possible to speak of such schemes as being either the same or different. In either case, the philosopher, who works within such a scheme, is at no disadvantage, for discussion is always possible within a scheme, and beyond this the possibility of disagreements and the possibility of resolving them are on the same footing.

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REVIEW

SEMANTIC ANALYSIS. By Paul Ziff. Ithaca, New York, Cornell University Press. 1960. x, 255 p. 55s. (Australian).

The skeleton of Professor Ziff's argument resembles in its outline that of a fish: the backbone supports a large set of parallel but unconnected appendages. This resemblance is intensified by the sub-division of chapters into numbered entries rather like those of a Wittgensteinian diarist. For while it is Ziff's chief aim to supply a method for deciding upon the meaning of linguistic elements, and to illustrate the method by answering such questions as 'What does the word "good" mean in English?', the shift from one entry to its successor often corresponds to a gap in the exposition of the argument. These gaps appear in an already gnomic—though often entertaining—prose, so that the book is most easily dealt with if the last chapter ('The Word "Good"') is read first, since contrary, perhaps, to theory, it can stand on its own. The other chapters should then be read twice—twice because knowing the output helps the reader to follow the input, though this procedure may not completely relieve his difficulties.

The method that the author wishes to use for determining the meaning of a morphological element in English has three steps. The first step consists in drawing up the largest practical list of utterances in which the element occurs. This list usually forms a part—or sample—of the element's *distributive set* provided that the items on the list are 'syntactically nondeviant whole utterances'. The distributive set for 'good' includes utterances like (1) 'She is a good woman', (2) 'That was a good shot', (3) 'What good is he?'. But a second kind of list is needed as well, and this list is part of the *contrastive set* of the element, i.e. the set of utterances in which the element may occur competitively with, or in contrast to, other elements. 'She is a happy woman', 'That was a lucky shot', 'What use is he?' are examples which contrast with (1), (2) and (3) respectively. Step number two in the procedure consists in discovering the relevant semantic differences (as against syntactic differences) between the relevant utterances of the distributive set on the one hand and those of the contrastive set on the other. To do this is to determine which conditions are associated with a given utterance of the distributive set and not with the competing utterances of the contrastive set. Thus, says Ziff, one condition associated with 'That is a tiger' and not with 'That is a lion' is that of being striped. The same difference holds in the case of 'I want a tiger' and 'I want a lion'.

By working through a series of such competing utterances we come to the third step. In it we attempt to characterize, for the element in question, the relevant differences between the two sets, that is, to distinguish a set of conditions associated only with utterances of the distributive set. In any but the simplest examples, this general characterization will be the result of suggesting various empirical hypotheses about the differences between competing members of the two sets. These hypotheses will be tested by finding out whether they explain—or fail to explain—the differences in distribution of the relevant linguistic elements. The upshot of this procedure is something like a dictionary entry, and like all such entries it is open to correction. Ziff remarks, for example, that the definition of 'inspect' given in the *S.O.E.D.* ('to look carefully into') is mistaken, for we can inspect carelessly. And what his remark brings out is his appeal in this case to majority usage, a usage which the dictionary misreports but may eventually alter.

Now, clearly, the discovery of the conditions in which a morphological element has meaning depends upon a prior discovery, that of the semantic regularities associated with the utterances in which the element occurs. A statement of a semantic regularity is 'a statement of a correlation between a type of event and a type of state of affairs', e.g. 'if "Pass the salt!" is uttered then generally salt is present'; or the regularity correlates two types of events, as in 'If "Hello!" is uttered then generally one person is greeting one or more others'; and so forth. Ziff uses the term 'projections' to refer to those utterances for which no relevant regularities can be stated, utterances that by virtue of their novelty or rareness in the language ('There is a purple Gila monster on my lap') or context-free character ('George K. crossed the Hudson at 2 a.m. on October 20, 1943') or references to the remote in space and time ('Caesar crossed the Rubicon') cannot be correlated with conditions that are *generally* satisfied. Instead, a projection correlates an utterance with certain conditions that are satisfied in a 'standard case', e.g. the condition that there is in fact a purple Gila monster on someone's lap is a standard case for the utterance 'There is a purple Gila monster on my lap'. Projections obtain their significance from the operation of the 'principle of composition'. This, says Ziff, is the principle which allows us to make and understand novel utterances because of our familiarity with their syntactic structure. Given two 'syntactically nondeviant whole utterance types' U_i and U_j, and two utterance types W_i and W_j which state the relevant semantic conditions that are correlated with U_i and U_j, then the principle is 'that the structural similarity between W_i and W_j be

a reflection of the structural similarity between U_i and U_j', where the latter two utterances have the former two as their exclusive correlates. One example given by Ziff is: 'The cat is on the mat' (U_i) may be paired with 'A philosophico-grammatical discussion is under way' (W_i), where W_i represents the particular set of conditions under which U_i is uttered. Then if 'The dog is on the mat' (U_j) is similarly paired with 'Some canine is on some mat' (W_j), the principle of composition requires that the structural similarity between U_i and U_j be reflected in a similarity between W_i and W_j. Since this is lacking here—for U_i and W_j are taken to be structurally dissimilar—it is simpler to pair 'Some feline is on some mat' (W_k) with U_i.

An obvious difficulty with this account of 'semantic correlates' lies in our knowing how to apply the principle of composition. The phrase 'reflection of structural similarity' offers little aid to anyone who wishes to make use of the principle in a given case. Ziff resolutely emphasizes this point, saying that he can only indicate in a vague way what is to be included under the title 'structural similarity' and that he can certainly provide no measure of the relationship. The linguist's lack of ability to provide a *general* characterization, however, does not seem to prevent him (in the person of Ziff) from coping with particular cases. It seems only to result in his not being able to supply a useful criterion to non-linguists. The difference, for example, between 'the orange flower' and 'the orange grower' is said by Ziff to be one of structure, since 'er' is a morphemic segment of 'grower' and not of 'flower'. Hence there is a change in the meaning of 'orange' from one phrase to the other.

Nevertheless, it should be clear that the notion of semantic regularities or projections offers no *new* criterion for determining, in any given case, whether a sentence is a significant one. For example, if 'Witchgren' is the name of a cyclone does it make sense to claim that Witchgren is on the mat? Both regularities and projections are defined in terms of the satisfaction of those conditions required for the utterance in question to be made, e.g. if the present utterance is not to deviate from a regularity 'there must be something named "Witchgren' that is being referred to by the speaker and there must be some situationally determined particular mat that the speaker is making reference to' (p. 116). But must not the required conditions also include one to the effect that Witchgren be the sort of thing that can be *on* a mat? If so, then determining the relevant conditions for making the utterance will either be identical with, or include, the stating of the conditions under which the utterance will be significant. And how will the notion of semantic regularity (or the principle of

composition) help us to *decide,* for example, whether a cyclone can be on a mat? How close does the resemblance to 'The cat, Witchgren, was on the mat' have to be in order for us to admit the new utterance about Witchgren, the cyclone? It is at this very point that Ziff's principle does no more than loosely characterize what we already know from our daily practice.

What these cases illustrate is that the method proposed by the author for establishing the meaning or significance of linguistic elements (in English) is that of the linguist. Its treatment of meaning in terms of the freedom of occurrence shown, with respect to each other, by certain parts of an utterance is taken from such sources as Zellig Harris's *Methods in Structural Linguistics.* The method is not one that could be practised by a philosopher unless he were willing to learn enough linguistics to be able to make a phonemic and morphemic analysis of his language. And it is not one that would be learnt by a philosopher in his professional capacity if he were not convinced that the method somehow strengthened his hand. Ziff's task, then, is to show us that it does. But consider some of the results of its application.

Ziff begins his discussion of proper names by saying that they are 'generally not words'; but the force of this remark remains largely unfelt because he has not, in his own phrase, 'tried to say or even to indicate exactly what a word is'. He says that he uses ' "word" in an ordinary way', yet what way is this? We can hardly rely on dictionaries if we bear in mind their corrigibility—nor can we be sure that Ziff is appealing to something called 'ordinary speech' or 'everyday discourse' since he elsewhere criticizes such appeals on two grounds: (1) they are irrelevant in the case of technical terms, (2) they neglect the fact that ordinary discourse often contains deviations from semantic regularities, e.g. irony. And even if we find these points not very persuasive—who, after all, do they affect?—it is still not clear that Ziff wishes to make the appeal or that it would help him were he to do so. For what is unusual about instances like 'The last word to cross his lips was her name—Lola'?

Ziff also denies that proper names ordinarily have either meaning or *a* meaning in English. He asserts that they do have a connotation, 'that a proper name connotes what is, as it were, noted with the name by the hearer', *e.g.* 'The Australian War Memorial' connotes a Canberra building. He goes on to say that a proper name like 'Witchgren' has meaning in English *if* (1) 'Witchgren is on the mat' and (2) 'A particular tomcat is on the mat' can be paired according to the principle of composition. Now one argument advanced by the author against this pairing

is that if there were another tomcat Glywellyn then (3) 'Glywellyn is on the mat' could be paired with the same utterance (2). Hence the pairings cannot be correct, *i.e.* (2) cannot be the meaning of (1) since (2) and (3) may differ in truth value. Still, it is by no means clear that the principle has been violated in this case. The author earlier says (pp. 65-66) that the principle 'does not preclude pairing one and the same utterance (or utterance part) with structurally dissimilar utterances'. Otherwise, he says, we should rule out informative examples like 'The dog is on the mat' and 'Some canine is on some mat', for both of these are paired with 'Some canine is on some mat'. In what way, then, does this argument show that the principle of composition has been violated in the case of Glywellyn? The point of this argument concerns a difference in truth values, not a violation of the principle. And even if such a violation were shown—as it is in fact by an additional argument—what could this way of putting the matter add to common knowledge? We do not need the principle in order to understand the deficiencies of the proposed analysis. Nor do we need it in order to account for these deficiencies: after all, the objections to ascribing a sense to proper names are well known, and rewriting these objections as infringements of a vague principle does not alter their character.

A large number of Ziff's arguments display this same kind of curiously idle motion. He maintains, for instance, that 'Nothing is both red and green all over' has a common interpretation which is synthetic and false, since a ball may be striped with red and green. He even discusses the question why the statement has been falsely thought to be analytic, as though he did not realize that the philosophical problem at issue has always concerned the quite different statement: 'Nothing is red all over and green all over at the same time'. Similarly, he argues that only words and phrases can be said, in general, to have a meaning. Utterances and sentences have a significance. This claim about English words may well be true. But what is the significance of a sentence? No clue is given; and the situation is made more obscure by the author's earlier comment that he cannot say 'what counts as a single sentence'. Again, the criterion of *salva veritate* is attacked as futile for determining whether two words differ in meaning, futile because it cannot even tell us if 'two occurrences of the same word differ in meaning', *e.g.* 'division' in 'The division is incorrect' and 'The division is happy'. The reason, however, why the criterion cannot be taken seriously is that its application requires the knowledge which it is supposed to establish. What is the point, then, of exposing a secondary defect in an abandoned position?

The chief illustration of the results to be obtained from Ziff's procedure is in his extended treatment of the word 'good'. After a detailed and interesting discussion of some one hundred and fifty utterances containing this word, Ziff concludes that 'good' in English means 'answering to certain interests'. The interests relevant in each case are shown either by the expression modifying —or modified by—'good' or by the context. However, it is by no means shown that the notion of good is not written into this definition. Does not 'answers to' here simply mean 'good for'? If 'a good theatre seat' means 'a theatre seat that answers to certain interests', how does this differ from 'a theatre seat that is good for certain interests'? The author has not indicated how they differ. If they do not, then the proposed definition sheds no light on any philosophical problem.

On the other hand, it is one of the desirable features of the last chapter, as of the entire book, that we are shown, though not perhaps intentionally, how minor a rôle the search for dictionary entries plays in philosophical work. On the evidence of the present volume it is an empirical task for linguists. No philosopher will be the worse for seeing how the task is done, but equally, this volume is not likely to convince him that he should do it for himself. That is, the author has not really succeeded in showing how adoption of his procedure answers to certain of the philosopher's interests.

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BOOKS RECEIVED

(Inclusion in this list neither guarantees nor precludes later review.)

CARR, Herbert Wildon. Leibniz. New York, Dover Publications, 1960. vi, 222 p. $1.35 (paper covers).

A well-known exposition of Leibniz, first published in 1929.

CHISHOLM, Roderick M., ed. Realism and the background of phenomenology. (Library of Philosophical Movements.) London, Allen & Unwin, 1960. viii, 308 p. 42s. (U.K.).

Extracts from Brentano, Meinong, Husserl, Prichard, Alexander, Russell, Moore and others, designed to illustrate the development of realism and its connection with phenomenology.

CONNELL, W. F. The foundations of secondary education. (Monographs on Secondary Education, no. 1.) Melbourne, Australian Council for Educational Research, 1961. viii, 127 p. 21s. (Australian).

CUMONT, Franz. Astrology and religion among the Greeks and Romans. New York, Dover Publications, 1960. xix, 115 p. $1.35 (paper covers).

First published in 1912. Originally the 1911-12 American Lectures on the History of Religions delivered at a number of American universities.

EDDY, W. H. C. Orr. With a preface by A. K. Stout and a prologue by R. D. Wright. Brisbane, Jacaranda Publishers Pty. Ltd., 1961. xxxvi, 764 p. 59s. 6d. (Australian).

Detailed analysis of the evidence in the court proceedings arising out of Professor Orr's dismissal from the Chair of Philosophy at the University of Tasmania, and other considerations, lead the author to conclude that Orr was innocent of the charges brought against him.

GIBSON, Quentin. Facing philosophical problems. 2nd ed. Melbourne, F. W. Cheshire, 1961. 63 p. 12s. 6d. (Australian).

A clear and readable discussion, for beginners, of the basic problems of philosophical method. First published in 1948, and now revised.

HARWOOD, A. C. The faithful thinker: centenary essays on the work and thought of Rudolf Steiner, 1861-1925. London, Hodder and Stoughton, 1961. 251 p. 31s. 9d. (Australian).

HILL, Johnson D., and STUERMANN, Walter E. Philosophy and the American heritage. New York, Philosophical Library, 1961. 254 p. $3.50.

"Our effort has three primary objectives. First . . . we desire to bring social issues within the scope of philosophic attention and to fashion a skeleton for an American philosophic perspective . . .

Second, we want to indicate how this philosophic creation may be used to fill a void in American education, whose vitality and richness have been sucked from it by an enfeebling naturalism . . . Third, alongside of the conventional rights of man . . . we undertake to place the right of suffrage." Preface.

LECLERC, Ivor, ed. The relevance of Whitehead: philosophical essays in commemoration of the centenary of the birth of Alfred North Whitehead. (Muirhead Library of Philosophy.) London, Allen & Unwin, 1961. 383 p. 42s. (U.K.).

Contributors are: Charles Hartshorne, William A. Christian, Frederick B. Fitch, A. H. Johnson, Nathaniel Lawrence, Ivor Leclerc, Victor Lowe, Gottfried Martin, W. Mays, Eva Schaper, Hermann Wein, Paul Weiss, William P. D. Wightman and Daniel D. Williams.

MASLOW, Alexander. A study in Wittgenstein's *Tractatus*. Berkeley and Los Angeles, University of California Press, 1961. xxii, 162 p. $4.

"My purpose in writing this book is merely to make a sensible, and, so far as I can, consistent interpretation of the *Tractatus* . . . I do not always follow the order . . . or give the same relative weight to the topics as in Wittgenstein's own presentation . . . In places I criticize and even reject some details of his view as I understand it." Introduction.

NEWMAN HISTORY AND PHILOSOPHY OF SCIENCE SERIES. London, Sheed and Ward, 1961:

No. 6. BUCHDAHL, Gerd. The image of Newton and Locke in the Age of Reason. 116 p. 5s. (U.K.).

No. 7. POCOCK, D. F. Social anthropology. 118 p. 5s. (U.K.).

No. 8. CALDIN, E. F. The structure of Chemistry, 49 p. 3s. 6d. (U.K.).

No. 9. HARRE, R. F. Theories and things. 114 p. 5s. (U.K.).

No. 10. CLARK, L. K. Pioneers of pre-history in England. 112 p. 5s. (U.K.).

A series of small paper-backs providing brief introductory surveys of their respective subjects.

PLATO. The Sophist, and The Statesman. Translation and introduction by A. E. Taylor. Edited by Raymond Klibansky and Elizabeth Anscombe. London, Nelson, 1961. vii, 344 p. 49s. 9d. (Australian).

Completed by Taylor in his life-time, but left by him in the manuscript stage, awaiting final revision. "The editors have limited themselves in the main to deciphering Taylor's hand and to reproducing his text."

POOLE, Reginald Lane. Illustrations of the history of mediaeval thought and learning. 2nd ed., revised. New York, Dover

Publications, 1960. xiii, 327 p. $1.85 (paper covers).
Re-issue of the second edition, originally published in 1920.

RUNES, Dagobert D. Letters to my teacher. New York, Philosophical Library, 1961. 105 p. $2.75.

SPINOZA, Baruch. Principles of Cartesian philosophy. Newly translated from the Latin by Harry E. Wedeck. New York, Philosophical Library, 1961. 192 p. $4.75.

The exposition of Descartes, *Renati des Cartes Principiorum partes I et II more geometrico demonstratae; accesserunt eiusdem cogitata metaphysica,* published by Spinoza in 1663.

VOLTAIRE. Philosophical letters; translated, with an introduction, by Ernest Dilworth. (Library of Liberal Arts.) New York, Bobbs-Merrill, 1961. xvi, 150 p. $1 (paper covers).

WIEMAN, Henry Nelson. Intellectual foundation of faith. New York, Philosophical Library, 1961. 212 p. $3.75.
Discusses Dewey, Brightman, Tillich and Barth.

ZABEEH, Farhang. Hume, precursor of modern empiricism: an analysis of his opinions on meaning, metaphysics, logic and mathematics. The Hague, Martinus Nijhoff, 1960. viii, 166 p. Guilders 12.50 (paper covers).

The author is critical of Hume's scepticism, and part of his theory of meaning, but concludes that "his two basic concepts, namely, the principle of priority of impressions to ideas, and the distinction between relations of ideas and matters of fact . . . become two known cornerstones of empiricism, the principle of verifiability and the principle of analyticity".

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EHRLICH'S SOCIOLOGY OF LAW[1]

By P. H. PARTRIDGE

At the beginning of Part III of *The Province and Function of Law,* Stone briefly discusses the provenance of the sociology of law. He refers to the view that there cannot be a sociology of law, distinct from other branches of sociology or from other social sciences, for the reason that law enters into virtually every sort of institution, or is an ingredient in every form and department of social organisation. Because of the omnipresence of law, it is argued, no *separate* science of law can exist; any sociological study of law must be absorbed into

> "an established science called either political science or sociology or by some other name. Legal institutions and processes are not *sui generis*; they are like other institutions and processes . . ."[2]

Thus, since every institution or association is regulated by norms and rules which make them what they are, any social scientist who specialises in the study of a particular type of association is engaged in the study of law, or of law-like norms and rules. He

[1] Eugen Ehrlich was born in 1862 and died in 1922. He was trained in Vienna, and spent the whole of his professional life as teacher of Roman Law in the University of Czernowitz in Bukowina. *The Fundamental Principles of the Sociology of Law* was published in 1913 and translated into English in 1936. *Freie Rechtsfindung und freie Rechtswissenschaft* (1903) is translated in part in vol. IX of the Modern Legal Philosophy Series. Ehrlich was a member of the "free finding of law" movement, but I have not touched on this aspect of his work in this paper, although it influences some of the argument in the *Fundamental Principles.* Other major works, including *Die Juristische Logik* (1918), do not appear to have been translated. There are two articles by him in the Harvard Law Review: *Montesquieu and Sociological Jurisprudence* (vol. 29, 1916), and *The Sociology of Law* (vol. 36, 1922—a swift summary of the argument of his book). Pound supplies an introduction to the English translation of *Fundamental Principles.* Max Rheinstein reviewed the book on the appearance of the translation in International Journal of Ethics, vol 48, 1937-8. There are brief and summary examinations in Friedmann *Legal Theory,* Gurvitch *The Sociology of Law,* and similar works. Stone in *Province and Function,* and Timasheff in *Introduction to the Sociology of Law,* refer frequently to particular aspects of Ehrlich's doctrine.

[2] Michael and Adler, *Crime, Law and Social Science,* quoted Stone, p. 392.

is studying also the factors which cause changes in the character of the rules; and the manner in which rules carry out their job of ordering the relationships among the men who man the institutions. How can there be, then, a separate or distinct sociology of law? Law cannot be detached from the innumerable and multifarious associations of which it is an integral part, and examined as something that exists in its own medium. We can study the sociology of law only as we study the state, the family, the trade union, or what not. This, of course, is not a view that is accepted by Austinians or by Kelsen, but they are not sociologists of law; they think of law as being a system of rules deriving from a single source. But, for the man concerned to study the "law in action", the argument may seem to carry some weight.

We might amplify the argument in this way. The collection of authoritative norms and rules which may be called law, in the sense that they regulate the behaviour of members of organisations towards one another, are too heterogeneous in character to be the subject of a single science of law. Sociology of law would presumably include a study of the sources of law—in the sociologist's sense of the manner in which particular rules come to be formulated or established as rules. And it would include a study of the validity or authority of law: i.e., of the psychological and other conditions which are necessary if rules are to be accepted as authoritative, as being really "the law". But when we consider all the different kinds of law that operate in a society (law so diverse as the customary law of a small community, the law that prevails in trade unions, the law merchant as it evolved in European societies, statutory law, the law of a written constitution, the law of evidence as known to common law judges, to take some examples at random), it seems unlikely that the sources and the conditions of acceptance or effectiveness in all these cases would be sufficiently related to lend themselves to any single treatment; or that any generalisations we could arrive at about the sources and validity of law would be very illuminating. According to this line of argument, we could argue that to talk about *the* sociology of law would be like talking about *the* theory of games, where the word "games" referred to a large number of very different activities, ranging perhaps from the small boy bowling a hoop and a little girl skipping to an Australia-England cricket test and the mile event at the Olympic Games. It might be pointless to treat all these different activities in a single subject, if they had practically nothing in common, except the accident of their being called by the same name.

Or take a real, not an imaginary example. Conceivably the

same objection might be brought against the "sociology of knowledge", the object of which is to demonstrate how our beliefs are conditioned or determined by conditions of social life, or, as Karl Mannheim often put it, how "social existence" conditions "consciousness". For the same kind of reason as in our other example, one may have some initial doubt as to whether there can be any single or coherent theory of the conditioning of "consciousness" or knowledge. "Knowledge" stands for so many diverse things (including religious belief, social ideologies, philosophical systems, hypotheses or theories in the natural sciences, the productions of imaginative literature, etc.) that one might argue that the characteristics which distinguish these things from one another are, scientifically, more interesting and important than anything they all have in common. And it does frequently happen that writers on this subject make pretentious and extravagant claims from which they are repeatedly forced to withdraw, but which they are unwilling to abandon (this could easily be illustrated from Engels' writings on "ideology", or in Mannheim's book, *Ideology and Utopia*). Engels and Mannheim both want to assert certain quite general hypotheses concerning the manner in which "knowledge" is conditioned by or reflects some type of social force or situation. The general hypotheses they assert are perhaps most strongly suggested and most easily supported by the study of political or social ideologies, but they seem to throw little light on the history of philosophy or mathematics or science, which, in their successive stages of development, are strongly affected by other processes—e.g. the obvious one of internal criticism; and for this reason sociologists of knowledge are forced by the facts to admit that the histories of many of the different branches of knowledge or belief must in large part be explained in terms peculiar to themselves, in their own technical language. In other words, only a philosopher can fully understand the history of philosophy; only a chemist can understand the history of chemistry; only a lawyer, that of law. But, of course, the sociology of knowledge appears to be a much less spectacular subject if one is compelled to admit that there may be an indefinite number of more or less unrelated "factors" or processes at work in the conditioning of knowledge or belief. For that reason, sociologists of knowledge are often reluctant really to abandon their claim to have possession of one big truth: to have discovered laws of great generality which can account for (or, at least, throw a great deal of light upon) the state of "knowledge" in a particular society at a particular time.

These general remarks are relevant to the discussion of Ehrlich's *Fundamental Principles* because it seems to me that his

concept of the sociology of law is open to the sort of objection that may be brought against the sociology of games or the sociology of knowledge, if those subjects are conceived in the way I have suggested. Ehrlich is wedded to a few fundamental principles to which he is frequently unfaithful but which he will never abandon. His most general principle he states in the Preface to the *Fundamental Principles*:

> "It is often said that a book must be written in a manner that permits of summing up its content in a single sentence. If the present volume were to be subjected to this test, the sentence might be the following: At the present as well as at any other time, the centre of gravity of legal development lies not in legislation, nor in juristic science, nor in judicial decision, but in society itself. This sentence, perhaps, contains the substance of every attempt to state the fundamental principles of the sociology of law."

In a sense, this sentence expresses a commonplace which no sociologist or student of law would wish to deny. We can take as settled many of the things which Ehrlich demonstrates with enormous learning: that social institutions, activities or relationships change constantly and independently of the activities of the jurist, the judge and the legislator: that associations, organisations and societies develop, to a considerable extent, their own customs, usages, rules or understandings—again without the assistance of the professional interpreters and makers of law. When law (in the sense now commonly accepted by lawyers and political scientists) is enacted, it is usually for the purpose of ordering activities, protecting and delimiting interests, and resolving conflicts of activities, claims or interests which have already established themselves within the "spontaneous" life of associations and organisations. Most men in any society live their lives most of the time not consciously affected by the "legal propositions" discussed by jurists, interpreted and applied by judges and formulated by legislators.[3] Just as (as Ehrlich says) "life would become hell if it were regulated by law alone" so the majority of men regard it as a disaster to get involved "with the law"; they do not expect to have to appeal to the courts to seek protection for some right or

[3] This is perhaps a suitable place to warn the reader that at least in one way (and doubtless in many others) the discussion in this paper does Ehrlich's book a serious injustice and will create a false impression in the reader who has not looked into the book for himself. Ehrlich writes from a very wide knowledge of legal history, the history of legal theory and of the characteristics of many legal systems; in his text, the theoretical or sociological principles in which he is interested are expounded in the context of very elaborate discussions of legal history. In this paper, I have separated the bare theoretical skeleton from the historical flesh, thereby unavoidably creating a false impression of the character of the book.

interest of theirs, and would regard the need to invoke the law as evidence of a regrettable breakdown in what they consider to be the proper and normal pattern of their lives. Ehrlich insists that we rely chiefly upon personal and social ties and sanctions of a kind quite different from those provided by the legal provisions in force in the community; and he provides numerous examples of this point, e.g. the relative unimportance of legal provisions and legal rights and duties in sustaining the pattern of marital or family relationships, or the relationship of creditor and debtor. The lending of money or the extending of credit would become impossible if there were a real risk, in many cases, that it would be necessary to invoke the law to secure compliance with contractual obligations. And, of course, Ehrlich would also be bound to deny that it is the *legal* sanctions 'in the background" which normally protect such relationships. Sanctions are important, but the most important ones are those deriving from the positions we occupy in the various associations in which we participate: most of us, most of the time, do what we have to do to maintain our position and satisfy our interests within this or that special association or context of social life.

I suppose that we would now accept such positions as these as being commonplace, at least when they are put in such very general language. On this side of his thought, Ehrlich is carrying on a polemic against several views which were once more influential than they are now. He is arguing, of course, against the Austinian tradition: the identification of law with command, and with the commands of a legal sovereign or of the State. But more widely, he is arguing against a practice of lawyers and teachers of law of identifying law with the rules known to jurists and judges, or those introduced by legislators: in other words, against the identification of law with "legal propositions", however the latter may originate, even if they should be deposited by custom. And thirdly, and most important in his view, he is arguing against something that usually goes along with this identification: the great exaggeration of the importance of that part of the law which consists of "legal propositions" in ordering or regulating social relationships. Legal propositions, Ehrlich wants to say, are only the exposed third (so to speak) of the iceberg which is the law: it is within the submerged two-thirds, consisting of what he calls "living law", that one finds the centre of gravity of legal development; and "living law" rather than "legal propositions" plays the largest part in the ordering or regulating of societies.

The influence of anthropological and sociological studies as well as the development of legal theory itself have caused these general propositions to be now pretty familiar and not very con-

troversial. What, I suppose, *is* controversial in his general theory is the account he gives of the relations between "living law" and the rest of law; and, in particular, what he says about the function of what I shall call "lawyer's law" in the general system of social regulation or control. It is especially in this respect that Ehrlich seems to me to try to straddle a number of different positions.

I shall now try to outline very briefly the bare bones of Ehrlich's sociology of law. How does he conceive the sociology of law? In contrast with the argument I outlined at the beginning, he holds that all the main varieties of law can be treated together, and do in a sense form part of a single system. For him the task of the sociology of law seems essentially to be this: to identify the main types of law that exist within any society (as he presents them, the differences of type are also differences of *level*); to show how the different levels are connected with one another; and to show how each is connected or interacts with the systems of activity or relations that make up social life itself. Broadly speaking, he distinguishes three *strata* within the whole body of law. *Firstly,* what he calls "living law": the rules, norms or codes which develop by practice or usage within the many associations of which society is composed, and which regulate the relationships of members of associations to one another in many cases "even without any formulation in words". This is the kind of law which Ehrlich regards usually as being "prior"—and not merely in an historical sense; in any society this is the bulk of law. Ehrlich frequently exhibits the priority that he gives to law of this type by speaking as if it were the whole of law: as when he insists again and again that we cannot learn what the law of a society is by reading the books of its jurists or its statute books, but only by discovering how its economic life, e.g., is actually conducted, how the members of families actually behave in relation to one another, how property is actually handed down from generation to generation, what sorts of contrasts or agreements are habitually made. It is obvious that Ehrlich is using "law" in a very wide sense, but it is a very deliberate use, for, at the beginning of his book, he attacks the professors and schools of law for teaching only the law which, as he puts it, regulates the behaviour of judges and state officials, but not the law which actually controls most of the lives of almost all the members of a society. And this old argument suggests one reason why he insists that living law is the bulk of law. For him, law consists of the rules etc. which actually "order" or regulate a society (a law which is not obeyed, he says in one place, is like a fashion that is not followed—a contradiction in terms); and it is the rules which constitute what

he calls "the inner ordering of associations" that mainly regulate a society.

No doubt Ehrlich can offer some strong arguments for including the "inner ordering of associations' as part of the whole legal system. The rules of "living law" are indistinguishable in function from many of the rules of "lawyer's law"; moreover, the latter could not exist or function without them—they all form part of an interlocking system of social control. This is the scientific sanction for using "law" in his very inclusive way: if we separate "lawyer's law" from "living law", we are almost bound to arrive at false views about the sources and the validity of the former (I am here using neither "sources" nor "validity" in its technical legal sense), as well as false views about the manner in which a society is regulated. Ehrlich's view is that we do not discover the sources of law by studying the growth of legal propositions but by studying the development of legal institutions.

I must add a little more about "living law", since it is here, according to Ehrlich, that we find the 'centre of gravity of legal development". He links "living law" very intimately with the life of associations. I cannot be certain that I fully understand all of his argument (whatever talent Ehrlich had, it was not one for careful, exact analytical statement), but I think that the following very free translation does not wildly misrepresent it. The conception we now have of what law is and of what it does is deeply influenced by our daily familiarity with statute law and judicial decision; therefore, we tend to think of law as consisting essentially (a) in verbal formulas and (b) in rules or norms developing externally to the persons or groups to which they apply—something applied *to* them or constituting an external standard to which actual conduct then conforms. A government, e.g., enacts a law relating to trade unions requiring them to elect their officers by secret ballot: the law is then a standard by reference to which the union organises one of its activities. Now, associations (e.g., tribal groups, the family, the household, a simple economic association consisting of master and apprentices) do not initially have to look to an external source for their law: they secrete law; in the course of carrying on their functions, they organise themselves in practice. But I think that Ehrlich wants to assert still more than this. He was influenced by Gierke, and there are Gierkeian overtones in much of what he has to say about associations and their law. He seems to think of the "natural' association in Gierke's manner as itself a living entity; so that the organisation which it evolves for itself—the rules and norms, the relations of domination and subordination, the rights and duties—is something that is created by the interactions of the members of the association:

that is to say, the "living law of the association is connected with, or is sustained by, the tacit and common recognition of rights, duties and functions which binds the association together. Now this is a conception which has pretty wide ramifications in his view. Since he holds that associations and their "living law" are the centre of gravity in legal development, it follows that, in dealing with the regulation of society as a whole, he is bound to minimise the importance of law as command, as the expression of power or domination. Certainly he cannot accept an extreme Marxist position but is bound to argue that law as the imposed rule of a dominant interest applies only to certain parts of the law and in certain limited spheres of the law's operation. Perhaps the appropriate image would be of law as something that is woven by the members of an association as they co-operate in their common business.

A qualification is required for what I said about the inclusiveness of Ehrlich's use of "law". Quite consistently he objects to the sharp division some theorists have made between legal norms and norms of morality, fashion, taste, etiquette. On the other hand, when he argues that associations create their own legal norms and legal relations, he is referring to a particular sort of norm and relationship, differing from moral norms and the others that more or less resemble them;

> "Wherever the legal norm attracted the attention of the sociologist . . . it has always been found in the company of other social norms. Nevertheless, there can be no doubt that there is an unmistakeable difference between it and the non-legal norms. It is as impossible to deny the existence of this difference as it is difficult, in view of the present state of the science of law, to indicate precisely wherein it consists . . ."

Ehrlich in fact attempts a distinction in psychological terms, or by reference to the "various overtones of feeling" which are evoked when a norm of each different sort is transgressed; he tries to convey the quality of feeling evoked by the breach of a legal norm curiously by employing the traditional legal conception of *opinio necessitatis*—the feeling, in the case of the departure from the legal norm, not merely that something that is usual or customary, but that something that is "required" or "obligatory", has not been done. This is not very illuminating, but it is important to notice here that Ehrlich wants to assimilate "living law" to other law (e.g. statute law) in certain respects: to show, not merely that below the world of statutes and other rules of law there are controlling codes and norms of *one sort or another,* but

that the "natural" associations of the community produce legal norms and legal institutions which have exactly the same feel of "legality" as the norms and institutions of state—and "lawyer's" law. One might expect the other differences between "secreted" law and "manufactured" law to be accompanied usually by differences of psychological tone; but perhaps anthropologists can detect in primitive communities different attitudes towards what is merely customary, what is in good taste, what is morally right and what is legally obligatory. I shall return later to the use Ehrlich makes of his notion of the legal and of legal relations.[4]

So much for the first level of law, "living law". The *second* type or level of law that Ehrlich distinguishes is "norms for decision".

> "Only that which becomes part and parcel of life", Ehrlich writes, "becomes a living norm; everything else is mere doctrine, norm for decision, dogma or theory . . . The first and most important function of the sociological science of law is to separate those portions of the law that regulate, order and determine society from the mere norms for decision . . ." (p. 41).

As the name implies, norms for decision are those rules, criteria or norms which are employed by courts in adjudicating legal conflicts. Ehrlich here, it will be seen, speaks rather contemptuously of norms for decision ("rules of law that have remained mere norms for decision, that become effective only in the very rare cases of legal controversy, do not take part in the ordering of the associations"); they control the behaviour of judges but not the normal behaviour of men in society; and this is one difference between his theory of law and that of the American realists, who tend to find the "centre of gravity of legal development" in the judicial process itself, i.e., in the creation of norms of decision. This, however, is not to say that Ehrlich does not regard norms for decision as playing an indispensable rôle in the system of law:

[4] There is rather more than this to Ehrlich's identification of the "legal". It is connected also with an obviously important doctrine that Ehrlich has concerning the "facts of law" of which I am not sure that I fully understand the import, and which I have passed over in the paper. By "facts of the law" Ehrlich means the "factual institutions that become legal relations in the course of legal development"; and he maintains further that the facts or types of relationship which create legal relations are four: usage, domination, possession and disposition of will, the last including several things—e.g., contractual agreement, testamentary disposition, and so on. I take Ehrlich to argue (but it is at this point that I am uncertain that I understand) that all law arises out of, or is connected with, these four fundamental types of situation; and Ehrlich sometimes seems to me to be implying that rules or norms relating to these situations are what we mean by the "legal". But there seem to me to be so many difficulties in connecting all law with one or more of these four "facts" that I doubt my reading of him.

but on this as on most other vital points in his doctrine, he frequently shifts his ground.

The *third* type or level of law is the "legal proposition"—one of his many descriptions of it is that it is "the precise, universally binding formulation of the legal precept". Here again, of course, as with "norms for decision", Ehrlich is concerned to emphasise the secondary or subordinate rôle of the legal proposition in the total body of law and in the ordering of social (and *legal*) relationships. There are legal relations before there are legal propositions: "the facts of law"—usage, domination, possession, contract, testamentary disposition—exist before there are general legal concepts and propositions concerning these things; generalisation and universalisation are relatively late and sophisticated refinements. One of Ehrlich's chief criticisms of Savigny and the other members of the historical school is that they supposed that law consists of legal propositions, and the legal propositions were the product of the popular consciousness. Norms for decision and legal propositions, however, are the creation of jurists or professional law-makers. Ehrlich, however, does not purport to be describing the historical stages in the development of law. In any society norms for decision and legal propositions are subsidiary to living law and legal relations. "Legal relations are created by society, not by legal propositions".

Quite properly, Ehrlich does not present these three types as being exclusive. The judge, seeking a norm for decision in a particular case, may find it in the usage or custom that has prevailed in the type of situation out of which the conflict has arisen. Or he may derive it from a consideration of the "concept" or nature of the association in which the legal conflict has occurred: cf. the controversy about the nature of a "church" that accompanied the Free Church of Scotland case, or, to take another example, the discussion that has recently occurred in Australia as to whether the master-servant concept is appropriate for university teachers. Or he may derive it from a legal proposition: the umpire in giving a batsman out "leg before" applies criteria specified in one of the legal propositions of cricket, the leg-before rule. Or he may formulate legal propositions himself in the course of rendering judgment. But living law can exist in society before the other two types appear; in any society there is always a great deal of living law which has never been brought into contact with norms for decision or legal propositions. Norms for decision may be independent of legal propositions: a judge can employ a norm for decision which does not profess the exactness and generality of a legal proposition. Throughout the history of law, one of the main tasks of jurists has been to scrutinise

decisions of judges in order to spell out the rules of law which they appear to establish. Ehrlich tries to show that, as a matter of history, legal propositions have normally grown out of norms for decision; and he argues in one place that the earliest examples of legislation we can find in the modern world appear to be norms for decision addressed to judges and other officials. This is in accord with his general thesis that the growth of law is always from the more particular and concrete to the more general or abstract.

I shall postpone for the moment an account of other parts of Ehrlich's doctrine. I wish to concentrate for a while on what I have already described. It is difficult to nail him down because he persists in claiming more than he is willing to defend; whenever he puts a card on the table you can be sure that within a few pages he will have snatched it away again. The most important example of this is his position concerning the "primacy" of "living law". There are many passages in his book where he goes a long way in asserting its self-sufficiency, and the secondary and derivative character of other elements in the legal order. On the other hand he has many other passages (e.g. about the rôle of the state in the legal order) which take the edge off his position. This is a typical example:

> "But also in times when the course of events was undisturbed" (he has been speaking of the rôle of the state in times of economic and political disturbances), "legislation has proved an indispensable instrument for the removal of antiquated institutions and the advancement of legitimate interests which are struggling for recognition" (p. 185).

In fact, his picture of the legal order is a rather complicated one; and I want to try to make it a little clearer.

In his *Legal Theory,* Friedmann makes this comment:

> "Ehrlich confuses the position of custom as a 'source' of law with custom as a 'type' of law. In primitive society, as in the international law of our time, custom prevails both as a source of law and as the main type of law. In modern state society it is still important in the first, but less and less important in the second, rôle. Modern society overwhelmingly demands articulate law made by a definite lawgiver . . . This confusion permeates the whole of Ehrlich's work" (p. 188).

But Ehrlich is not guilty of any such confusion. He is quite clear that custom (or "living law" which, as I have said, may not be quite coextensive with custom) is *both* a main source of "lawyer's

law" and the main type of law. The difference between Friedmann and Ehrlich is one concerning a fact—the contemporary importance of custom as a type of law; and Friedmann denies what Ehrlich asserts. It may be that Ehrlich is inclined to fall into the historian's fallacy. Because he can discern in the history of law norms for decision and legal propositions growing out of the soil of spontaneous associational life, he is too apt to leap to the conclusion that the functional relations apparent in earlier stages of growth must be substantially the same in any society and at any stage. Even this is perhaps not quite fair criticism; he marshalls a great deal of argument and evidence from modern societies to support his main propositions: that the law of the two higher levels touches only very lightly the complex order of society; that the legal order changes perpetually; and that it is impossible for state law to keep pace, or to profess to regulate more than a small fragment of the web of social relations.

But Friedmann might have put his argument differently. He could have asserted (a) that the trend is for custom to be supported or supplanted more and more by law (in the narrower sense). Custom is now usually felt not to possess sufficient authority, but to require the authoritativeness conferred upon it by state- and judge-made law. And (b) while custom may still be important as a source of law, nevertheless it is not created or adapted quickly enough to perform the functions it could perform in simpler, less progressive or unstable societies. The trend is, therefore, for law to expand and proliferate to fill the ever-widening gaps that "living law" cannot fill.

Neither of these contentions is likely to be attacked. There are now very few of the main structural relationships of our associational life which have not either been legislated for, or been the subject of litigation and judicial pronouncement. And, in our society, legislation continually brings new types of association into being or creates new types of legal relations. Ehrlich recognises all this,[5] but he does not seem to admit that it is incompatible with the particular emphasis he gives in his account of the legal order. If I understand him correctly, he wants to argue that, important as "lawyer's law" may be, associations new and old continue in large measure to evolve their own structure; fundamentally, the rights, the duties and the functions men have, the kinds and degrees of consideration they receive or expect to

[5] But cf. such a passage as this (p. 356): "If English law should be introduced everywhere on the Continent of Europe, the family, the corporations, ownership, the real rights, would remain what they had been until then, and even though they should be adjudged according to English law, they would not become English legal relations. Legal relations are created by society, not by legal propositions."

receive, the "legal" relations by which they are bound, are evolved within this or that association. Norms for decision and legal propositions eke out the "living law" and may often transform it; they provide norms and rules for dealing with trouble situations concerning which "living law" has nothing to say; in this and many other senses "lawyer's law" plays an important creative rôle. But *how* important is this fact? Ehrlich rests a great deal on the argument that the trouble situations which attract the attention of the legislator or the courts play a small part in the life of the ordinary man: "the relation that has never come into contact with legislation and judicial adjudication is the normal relation". Ehrlich does not deny the facts to which legal positivists point; he believes, however, that from these facts they draw superficial and false conclusions about the structure of the total legal order.

However, in spite of Ehrlich's apparent inconsistencies or shifts of position, it is possible to construct a model that represents his conception of the legal system. It is clear that, in general, he wants in some sense to emphasise the primacy of "living law"; to maintain that the *legal* ordering of social life is mainly provided for by the usages, rules or norms which the associations and groupings of society create for themselves; and that the other levels of law, norms for decision and legal propositions, somehow are to be understood only in relation to this ultimate substratum—"the order of human society is based upon the fact that, in general, legal duties are being performed, not upon the fact that failure to perform them gives rise to a cause of action"[6] "The legal norm according to which legal disputes are being decided, the norm for decision, is merely a species of legal norm with limited functions and purposes".[7] Nevertheless, his model of the legal system does not at all resemble the foundation-superstructure model of the Marxists: for Ehrlich, law in the narrower sense is not an emanation from or reflection of any social foundation, and he is not committed to any single-directional theory of causal influence. His model rather resembles two circles, an inner and an outer which webs it round. He himself sometimes speaks of the "first" and the "second" legal orders. The first legal order, the inner circle, refers to the core or web of social relationships, transactions, usages, norms and rules which have arisen in society itself, the inner order of associations, the "living law". Ehrlich has been accused of identifying the whole of society with the legal order, the whole of sociology with the sociology of law;[8] whether this is so or not, he insists on maintaining that the actual forms of

[6] *Fundamental Principles*, p. 23.
[7] *Ibid.*, p. 24.
[8] Kelsen, *General Theory of the Law and the State*, p. 26 ff.

relationship that prevail within associations and within society is a legal order: "Legal relations are created by society, not by legal propositions." What surrounds this inner circle, the second legal order, is the system of legal conceptions, norms for decisions, rules or legal propositions created by courts and tribunals or enunciated by the legislator.

Now, one would expect that the interest and importance of Ehrlich's contribution to the sociology of law would be found mainly in his more detailed analyses of the interrelations and interactions between "living law" and law of the second order. What would be of most interest for the sociologist and historian of law would be the relationships, casual and other, between the two circles which Ehrlich is able to establish. In fact, it is by no means easy to distil a clear or systematic account of these relationships from the lengthy, learned but diffuse discussions of the *Fundamental Principles*. One may doubt whether he is always consistent; it is also doubtful whether the effect of his discussion is to confirm his own central sociological point which is, it will be remembered, that the centre of gravity in legal development is to be found in society itself. Put thus, this statement may be argued to be vacuous; if, somewhat more concretely, it is taken to mean that the centre of gravity for all legal development is to be found in changes in the "living law", it can hardly be said that Ehrlich's argument and his mass of historical illustration clarify or substantiate the position.

His exposition, in fact, brings out the diversity of relationships between what he calls "living law" or social usage and the legal framework of the second order. Frequently, in accordance with his general thesis, he is of course concerned to emphasise the independence of the relationships, usages, rules, norms of ordinary life from the rules and norms known to and applied by the lawyer: he returns again and again to the point that it is only exceptionally that the law of the courts or of the state impinges upon the transactions of ordinary life. Again, it is frequently suggested that, as regards the norms for decision, i.e. the criteria or rules applied by the judge, these historically or genetically are based upon the relationships acknowledged by "living law": "every norm for decision is based upon this inner order"[9] (i.e., the inner order of associations which they create for themselves). But this, Ehrlich argues, is far from being the whole story. The norm for decision "is always something more than and distinct from the inner order of the association"; and this is so for a variety of reasons—the relationship which has produced dispute

[9] *Fundamental Principles*, p. 123.

and invited the attention of a tribunal is something different from the same relation "at peace"; the fact that trouble cases have occurred within the inner life of the association shows that a situation has arisen which the "living law" has not anticipated and for which it has no norms or procedures available, and so on: the associations themselves, therefore, require that tribunals, jurists and legislators shall evolve norms of decision for their completion and perfection.

Equally with legal propositions, the more general and abstract rules of law:

> "The norms prescribed by the legal proposition therefore can either secure absolute enforcement for the norms that flow from the facts of the law, or they can hinder them or invalidate them; and, lastly, they can attach legal consequences that flow from the facts. Consequently the legal order which society self-actively creates for itself in the facts of the law . . . is brought face to face with a legal order which is created by means of legal propositions, and enforced solely by means of the activity of the courts and the other tribunals of the state. And norms, rules of conduct, flow from this second legal order no less than from the former to the extent that it protects, gives form and shape to, modifies, or perhaps abolishes, the facts of the law."[10]

Consequently, in the development of the legal system, and thus in the shaping of the pattern of social relationships, Ehrlich does after all give very considerable weight to the relatively independent activity of the jurists, the courts and the legislator—for he insists that legal propositions, and indeed most of the law of the second order, is the product of the specialised law-making agencies. One of his chief criticisms of Savigny and the Historical School with which of course he has much in common is that that School, according to him, supposed that the "popular legal consciousness" could produce or express itself in "legal propositions", abstract, highly generalised, rules of law.

Let me now raise one or two general questions concerning the value of Ehrlich's conception of the sociology of law and his model of a legal system. Is this a model of the legal order in society which is very useful either for the sociologist or the lawyer? Does it serve one of the chief functions of a model, in helping us to plot in more detail the different components of a

[10] *Fundamental Principles*, p. 197. But cf. p. 356 ". . . this inner order from which the judicial system derives *the greater number* of its norms for decision, which juristic science universalizes, subsumes under the juristic concepts, or construes with the help of the latter . . ." [my italics].

legal system, their functioning, the relations between them? Or should it be discarded as misleading in some fundamental respects, either when applied to societies in general or (as Friedmann thinks) especially when applied to Western societies?

Although it is sometimes said that Ehrlich is one of the founders of the sociology of law, the *Fundamental Principles* does not seem to have influenced very much the work of more recent sociologists or lawyers; in so far as his work has been referred to in more recent books, it is usually in reference to his attempt to include within the concept of law the whole area of ordered social life. His main sociological point, viz., his emphasis on the complex of social relationships, rules, norms and codes which regulate social activities more or less independently of the coercive rules of the legal system is, I think, a sociological commonplace. The argument that social activities or interrelationships find their own adjustments and do not have to be legislated for, that rules and norms often grow out of activity, that there are contracts before there is a law of contract, that enactment by the state, or by any other specialised or centralised law-making organ is not part of the very concept of law. This line of argument may be effective against some theories concerning the nature of law and of its rôle in social regulation that may have been held from time to time, but they do not need to have been strongly contended for at any time within the present century. But perhaps the main weakness of Ehrlich's book as a theoretical construction is the lack of any single, consistent, very definite hypothesis concerning the structure or the mode of development of a legal system. It is true that he makes the foundation of a legal system the separate "associations" that exist within a society, and that he puts their inner ordering, or the body of "living law" which associations generate, at the centre of the legal system. This is the central point of reference. But it turns out in fact that associations and their inner ordering explain less than we are initially led to expect. It is not only, as we have seen, that norms for decision and legal propositions are developed to supplement, modify or even abolish the inner ordering that the special associations and groupings have produced for themselves. We also learn[11] (from what Ehrlich says about the state and its functions) that associations form part of a more inclusive society; that the state imposes uniform requirements, arising out of the will or needs of society, upon the constituent associations; Ehrlich even insists that associations may conflict and become engaged in a struggle for dominance, and that the law enforced by the state upon all associations and their

[11] See especially chapters VII, XVI and XVII.

members frequently expresses the relations of domination and subjection that have emerged within society itself. These are propositions that no social theorist will want to deny; yet we are left asking whether, once they have been admitted, anything important has been done when associations and their self-created inner orderings are put at the foundation of the legal system.

Yet, in spite of the qualifications and inconsistencies, it is still true that what is distinctive about the sociological model of a legal system that Ehrlich presents to us is the special place he attributes to "natural" associations and to the complex of rights, duties, reciprocal functions, rôles, etc., which they create as part of their "inner ordering". I think it could be argued that, in so far as Ehrlich's account of the growth of a full legal system is controlled or influenced by this initial hypothesis, he is led unduly to minimise certain very important factors in modern political and legal development. As the state becomes more active in social regulation, and citizens become more accustomed to protecting claims and interests by means of state action, they tend to think increasingly of securing their claims through legislation. Ehrlich of course makes the point that the existence of law and the administration of justice precede the growth of the state as a specialised institution; he also says what other writers have often pointed out, that it is only in the last 150 years that the view has come to be commonly accepted that it is a function of the state to legislate constantly. But he does not explore sufficiently the possible significance of this historical development. It is possible to argue that, from the point of view of legal theory, the distinguishable associations within society to which Ehrlich attaches so much importance are growing much less important as separate or relatively independent areas for the creation of social controls: this may be an aspect of the disintegration of the smaller social groupings and communities about which contemporary sociologists are writing so much. It may be a very important fact that ours is a society in which there is a very wide *popular* feeling for something resembling the Austinian conception of law, as we might put it: every butcher and baker is a legal positivist now in the sense that he vaguely thinks of law as something necessarily made by the state and of the state as something whose business it is to make law. If this is so, what are we to say about the "popular legal consciousness" of which Ehrlich, under the influence of the Historicists, often speaks? He seems to assume naturally enough that the "popular consciousness" will find its expression in the actual inner orderings of smaller associations and social groupings. No doubt it will always be true that, when men regularly deal with one another in face to face relations in

organisations or in small communities, they will directly create a great deal of what Ehrlich wants to call "law". But it is also true that, with the growth of democracy and all its accompaniments, the "sense of the state" that people possess has grown a great deal stronger: i.e., the sense that people have of their own interests, and of the way they are related to or threatened or obstructed by other interests throughout the society. And this appears to be connected with the idea of the state as the chief resolver of social maladjustment, or the chief regulator of social relationships.

This is only to say that Ehrlich, in the manner in which he concentrates his attention on the diversified groupings of European societies (it is clear that his thinking is strongly influenced in many respects by the cultural diversification of the Austro-Hungarian Empire of before 1914) does not explore very seriously the changes that are accompanying the democratisation of modern societies. In the democratic or "open" society, there is, firstly, a heightened awareness of their interests among social groups and organisations; secondly, a readiness to organise for the protection of group interests; thirdly, an increasingly sensitive awareness by social groups of their position vis-à-vis that of other groups; fourthly, and in consequence of these other developments, an attachment to such norms of legal development as distributive justice, social equality, equality of opportunity, etc.; and, finally, a stronger determination to use the state as the main agent of adjustment or realisation. Again, the extension of publicity in modern democratic societies is clearly of very great importance for the sociology of law. We find in contemporary democracies (a) a stronger sentiment or sense of the public as the subject of political and legal activity; (b) a common belief to the effect that "the public interest" must prevail over other interests in cases of conflict; (c) a constantly and rapidly expanding area of personal and social activity which is regarded as being "public", in the sense of being of public concern or affecting the public interest. One could easily provide illustrations, ranging from public interest in and control of the conduct of industry, or the treatment of the internal organisation and policies of trade unions as being matters that affect the public interest and legitimately invite public intervention, right down to the now accepted invasions of "privacy" by the reporters and photographers of the popular Press. And, of course, those groups whose fortunes, careers or power derive ultimately from their "services" to the public (including politicians and journalists) have a direct interest in encouraging the expansion of the sphere of "the public" which I am now considering. (But this is an oversimplification: there are many influences at work

affecting the struggle between "publicity" and "privacy", including the influence of surviving traditions of privilege, autonomy, and so on).

So democracies are "open societies" in another sense of the phrase than that which Popper made current:[12] in the sense that all associations and organisations, all areas of social activity tend to become subject to public inspection and interference. Indeed, one might say that there is a curious conjunction of the two different kinds of "openness"—Popper's sort and this other: the pluralism of groups, associations or organisations; the pressure of public vigilance and demand, the deference to public opinion, the insistent assumption that the "public interest" must be protected and given effect to. These produce many of the characteristic features and issues of modern politics. However, I do not want to go any further into this point in political theory; I shall now try to bring what I have been saying to bear on Ehrlich's model of the legal order.

The relevance of the expansion of the sphere of "the public" as contrasted with those of the private and the privileged for an appraisal of his model is pretty obvious. The pressure of external, public demands and interests upon an organisation tends to diminish the power of producing its own order or law; it becomes more subject to imposed or negotiated adjustments. Generally speaking, what comes to be considered as being of a public character is also assumed to be ripe for political or state action. The habit of considering so much of what is done within a community (including so much of what is done within associations or organisations) as going on within the all-inclusive context of the public sphere, as affecting the interest of the public, lends very great importance to certain particular mechanisms of social control. For example, very great importance comes to attach to the process of legislating for rather detailed adjustments between the clashing interests of individuals, groups and associates. (It is interesting that almost the whole of contemporary political science takes it as being almost self-evident that the prime function of the state is necessarily to engineer adjustments of diverse and conflicting interests). Again, it is commoner in contemporary societies than in most earlier ones for norms or standards represented as being required in the public interest to be imposed by legislative or administrative action upon the life of associations. A great deal of 20th century trade union legislation—such statutes as the Wagner Act, the Taft-Hartley Act, the British Act of 1926—provides good examples. There is an abundance of illustration: in a

[12] Popper, *The Open Society and its Enemies.*

different field, Child Welfare legislation, in so far as it imposes norms of conduct upon parents within the family, is another case in point.

If, now, we return to Ehrlich's model, I think we must say that it is essentially misleading. In putting the centre of gravity of legal development where he does, he has probably been too much influenced by his study of earlier phases in the growth of law. As we have seen, in saying that the centre of gravity lies in society itself, he means in the "primary" associations (but "primary" is not his own term) or organisations of society, in the legal institutions and relations they create within themselves, in their own "living law". He does not say that lawyer's law is wholly derivative from that, though some of it always is; but he would claim, at any rate, that the *content* and the *function* of much the greater part of law at any time can be understood only by referring to associations and their inner order. This is his central point, the programme of the sociology of law as he understands it. My criticism has been that "the inner order of associations" will not carry anything like the load he puts on it in his development of a theory of the sources, authority and functioning of law in general.

Nevertheless I think that Ehrlich is still an interesting and important writer. In pointing to the network of associations, to the law-like relations of associational life, to the varied possible connections of other forms of law with these, he was working on *one* very important theme in the sociology of law. Legal sociology will always have a big field for inquiry in examining how groups or associations create their own norms, how these norms sometimes become embodied in law in the lawyer's sense, how legal norms interact with norms and usages of sub-legal social life. Moreover Ehrlich opened up a very important line of inquiry in his attempt to distinguish different *types* of legal norms and legal rules; but I am incompetent to say how tenable or useful his fundamental distinction between "norms of decision" and "legal propositions" might be, since this is a part of the sociology of law which clearly must be privileged ground for the lawyer. However, I should be astonished if this general type of inquiry on which Ehrlich insists should not be a very central one: Ehrlich with his mass of historical argument and example makes it seem very plausible to the layman that there *are* different forms or types or uses of legal norms and rules, and that these are rather differently connected with social conduct.

Of course, a number of different lines of criticism could be opened up against him. The one I sketched in the last few pages was intended to show simply that—at least so far as modern democracies are concerned and, still more, totalitarian states—he

exaggerates the independence and creativeness of associational life in sowing the seeds of new law. To make Ehrlich's model less misleading, one would have to develop other themes of the sociology of law which he touches on in only a naive and sketchy manner—in a manner that ignores serious political and sociological analysis. He has no very detailed conception of the manner in which individuals, groups and associations are more and more geared to the general, public mechanisms of adjustment, law-making and social control. Recalling for a moment my image of the inner core and the surrounding tissues of judge- and state-made law, I don't think that this is an aid to understanding either the genesis or the function of a great deal of the law characteristic of contemporary communities—e.g., the development of law relating to compensation for injuries, or the whole mass of social welfare and social security law.

This criticism is connected with a very far-reaching deficiency Ehrlich exhibits as sociologist of law. As we say, he attacks Savigny for having believed that popular legal consciousness could create legal *propositions*; he attacks almost everyone else who has written about law for holding that law is to be identified with formulated or explicit norms, rules or propositions. Yet, even though he insists that norms or rules may be expressed often only in usage, in mode of behaviour and emotional reaction, his own discussion of law proceeds almost entirely in terms of norms and rules, implicit or explicit. He stands all the time, almost, on this particular crust. And, in anything he has to say about *persons* who are concerned in the growth of law, he rarely considers anyone (apart of course from the associations themselves) except professional law-makers—jurists, advocates, judges or legislators.

In other words, he is not sociologist enough really to explore the links between law and the associations. Groups and associations differ in many ways, including their capacity to produce living law, and their relations to lawyer's law. There are some associations which, because they have a long tradition of autonomy and for other reasons, provide some rich examples of the living law Ehrlich was interested in and of some of the relations between the "outer skin" and the inner order that he discusses. Universities are an obvious example of this. At an earlier time in social history, the household, especially when it was a more or less self-contained productive unit, provided many examples of Ehrlich's thesis concerning associations and their living law. But what should we say of the modern capitalist factory, or of the large business corporation? It is clear that not all associations are in the same position; that some more than others are "public" bodies in the sense that their activities affect "public" interests, attract public attention,

and are subject to legislative or other official legal control or intervention. Ehrlich of course by no means ignores all the social influences leading to an extension of the act of "state law" and a restriction, modification or abolition of the inner law of associations and organisations. But his interest was mainly in the social and legal rôle of social law, and he does not really set himself to examine the question whether the peculiar rôle he attributes to associations and to social law, as distinct from the other levels of law, is a permanent or universal characteristic of any legal system.

It is perhaps also obvious from what has just been said that Ehrlich's sociology of law suffers from being written at too general a level: it does not seem altogether profitable to try to formulate in general terms the relations between social life and legal propositions, and for all societies. Societies differ greatly from one another in respect of those characteristics in which Ehrlich was interested: there is a great difference between societies permitting a large degree of racial, cultural or legal pluralism and those which are highly centralised and totalitarian. Even within the one society, the prosecution of Ehrlich's programme for the sociology of law would require one to study the different forms, functions and operations of different associations and organisations, and their quite different relations to public institutions and to the law. It is curious that a writer who made so much of associations as Ehrlich did should not have been more interested in the differences between associations and in their different rôles within a legal system. However, if we were to pursue this train of argument, we should probably return to the point that was made at the beginning of this paper, viz., the point about the impossibility of there being a single subject—*the* sociology of law. Laws emerge or are shaped in many different ways; "the law" bears very differently upon different departments of social life. At least, it seems very plausible to suppose that the Sociology of Law could never be other than an accumulation of innumerable, diverse studies of processes and institutions which are related to one or another of the many different types of law. In a sense, this is what *The Fundamental Principle of the Sociology of Law* is; it is not an advantage that Ehrlich's learned and diverse discussions are made to fall within the framework of what purports to be a single, systematic theory of the nature of a legal system and of the factors controlling the development of law.

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COUNTING

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Russell says,[1] ". . . what we do when we count (say) 10 objects is to show that the set of these objects is similar to the set of numbers 1 to 10. The notion of similarity is logically presupposed in the operation of counting, and is logically simpler though less familiar". It follows from this that our understanding of number is not to be found in counting but instead in the logical concept of similarity; and so we get the definition of number expressed in terms of it: "The number of a class is the class of all those classes that are similar to it" (p. 18).

It is true that the notion of similarity is logically presupposed in the operation of counting; but it seems also to be true that the operation of counting, or at least the concept of number, is logically presupposed in the notion of similarity. For in this technical sense of 'similar', two classes are similar if they can be put into a one-one correspondence with each other; that is, if the members of one can be paired off with the members of the other in such a way that none are missed and none dealt with more than once. Thus, we have to recognize, or understand what is meant by saying, that there are *two* classes here, not just *one*. We further have to understand what is meant by *pairing* off, and we have to know what it means not to miss any (we must not leave out of our correlating *one*, or *two*, or *three* . . .) and what it means not to use each member more than once (we must not use *one* of them, or *two* of them, or . . . , *twice*, or *three* times, or . . .). If we did not know how to use numbers how should we know that

o	oo	o	o
xx	xxx	xx	xx

is not one-one correlating the circles with the crosses?

But if it is true that counting and similarity presuppose each other, they are logically inseparable; and this means that an explanation of counting must enter into the logical foundations of arithmetic. To leave it out and to consider only similarity is to give a partial account. To bring it in, on the other hand, will be to define number in terms of both it and similarity, if number can be defined.

I want therefore to look at what is involved in counting and to distinguish what I shall call the counting rhyme, counting and controlled counting. In doing so, I shall look at how a child might be taught to learn these things and it may seem that I am

[1] *Introduction to Mathematical Philosophy*, Ch. I, p. 17. Unless otherwise indicated all references are to this book.

trying to give a genetic or psychological account. This is not my purpose. I look at the learning situation for one reason only, and that is to try to answer the question: what must be known before we can understand number? For I think that the answer to this question tells us something about what concepts must, logically must, enter into, or be presupposed by, the concept of number. The answer I want to give is that we must know the counting rhyme before we can learn to count, and we must know how to count before we can learn how to control it, and we must be able to control the counting before we can understand number. So, it would follow, the concepts involved in the counting rhyme, counting, and controlled counting, are logically presupposed in the concept of number.

In saying this, however, I do not want to commit myself to the view that we must know all the counting rhyme before being able to count at all; or that we must be able to count with complete understanding and without making mistakes, before we can learn to control it. To say this would be to say something about the psychology of learning and, I imagine, it would be false. No doubt in an actual learning situation a child will learn some of the counting rhyme and then how to count partially (*i.e.* he will relate counting to *this* situation but be unable to apply it in another) then more of the counting rhyme, and then how to control the counting in this situation (but not perhaps in this *sort* of situation), and so on. The actual process will be complicated and mixed up, different parts of it may be learnt simultaneously; but it is not these kind of threads that I wish to untangle. And perhaps this will serve to distinguish what I am doing from a genetic account. Perhaps it will be clear, too, that I am not so much concerned to define number as to understand it; or perhaps, to understand our understanding of it, for we all understand it perfectly well.

* * *

It is easy for a child to learn the counting rhyme. By this I mean that a child can be taught to say the numerals 'one', 'two', 'three', . . . , until he catches on to the fact that there are certain key words ('twenty', 'thirty', . . . , 'a hundred', . . .) and certain repetitive patterns (after 'thirty' you say 'thirty-*one*', 'thirty-*two*', . . . and so on). It is rather like learning those special kinds of nursery rhyme which are composed of repetitions with minor twists, some of which are such that the last line leads back to the first and the whole thing can be repeated indefinitely. So the child learns an endless routine by catching on to the rather complicated rules about repetitions.

These rules for repetitions are important. For they are such

that (when the child has learnt them all) there is never any doubt about what *numeral* comes next. It is always a different numeral and there is always a recipe for its construction. The child who is in doubt about what comes next after 'one thousand three hundred and twenty-three' has not yet learnt the rhyme because he has not yet learnt the rules in terms of which the rhyme is constructed.[2] So it is with the child who thinks that there is no next numeral, that this is the end.

These rules, which are rarely if ever stated explicitly, are important because there could be no counting without them. But the child who has learnt the counting rhyme has not yet learnt to count. The difference between the counting rhyme and a nursery rhyme is that the former can be used, it can be applied; and when it is applied correctly—that is, in accordance with more (though different) rules—the child can count. In this way:

We show a child a finite collection of objects, say the following, x x x x x

and we instruct him to point to (say) the left-hand object and say 'one'; next we tell him to move his finger to the adjacent object on the right, point to it and say 'two'. And we explain that he is to carry on in this way, saying the numerals he has learnt in the rhyme and pointing to the objects in turn. He makes mistakes and we correct him until he has learnt to associate (one-one correlate) his counting rhyme with his moving finger. Each numeral (the spoken word) corresponds to an action (pointing). The numerals go on indefinitely in terms of the fixed pattern which he has learnt (the rules for *saying* the rhyme correctly) and the actions can go on indefinitely even with a finite set of objects. Thus we might say to him: when you get to the end and say 'five', start at the beginning again and say 'six', and then carry on in the same way. Now he can count; and he can go on counting for ever. His endless verbal routine has been associated with an endless pointing routine. We do not need an endless set of objects for him to go on endlessly; all that we need is an action, or a set of actions, which can be repeated endlessly.

Counting, then, is one-one correlating numerals and point-

[2] The question, 'Does he know *all* the counting rhyme?' or 'Does he know *all* the numerals?' can only be answered if it is taken as, 'Does he know the rules in terms of which the rhyme is constructed?'

[3] Counting, as here described, should not be confused with *counting the objects* in a group; that comes later (see below) and is what Russell is concerned with in the quotation at the beginning of this paper. For Russell, counting the objects is one-one correlating numbers and objects, certainly not numerals and pointings, but since we are at the moment talking about something different we are not yet disagreeing with him. Perhaps one could ask here, however, how exactly one could correlate Russell's numbers (which are classes of classes) with objects.

ings.[3] But this is to describe it, not to indulge in it; and this is misleading. A child may learn how to correlate numerals and pointings, even though he has not yet got the concept of number, just because he is learning how and not learning that. One-one correlating at this stage is performing; it is pointing and saying correctly. So though he knows how to correlate he does not know that it is correlating. To describe it, however, is to say what it is; and this is to presuppose a knowledge of number.

Another reason why Russell wants to rule counting out of the logical foundations of arithmetic is this: "In counting", he says, "it is necessary to take the objects counted in a certain order, as first, second, third, etc., but order is not of the essence of number; it is an irrelevant addition, an unnecessary complication from the logical point of view" (p. 17). And it is true, as we have seen, that counting requires an order, and it is true also that number is independent of order; but this is not to say that counting does not come first, logically; it is only to say that when we have taught the child to count, we have not yet taught him what number is. That comes next; temporally and logically.

We say to him: if you want to know *how many things there are,* count (point and say, beginning at the beginning) until you come to the end; then stop. What you say then ('five' e.g.) is the number of objects there. That is what we say; and that is what 'the number of objects there' *means* for him. Why do we say this? Because the objects are distinct, the numerals are distinct, and the objects are linked to the numerals by the distinct pointing actions. The actions are a mediating device which give the numerals a use. Later, the actions become different. We look at each object in turn instead of pointing to each in turn; but the actions, though no longer overt, are still there. *'Count the objects',* then, means, '*Count* (point and say) *and stop* (when the objects run out)'. The actions never run out and counting need never stop. We have thus added something to counting when we get to numbers. We have controlled it; given it a use. We have shown how it can be used to answer the question 'How many?'.

Order is still involved in this, but we next show that order is irrelevant to 'the number of objects there' by getting the child to start at the other end and stop when he gets to what was the beginning. He ends up by saying the same numeral. In the process he discovers, too, that numerals are not names; 'one' is not the name of the first object any more than 'five' is when he starts from the other end. So he discovers that the numeral, when used in counting objects, is a technique word which, when the counting is stopped, is by courtesy used as a group-characteristic word. We now say that there are five objects there,

or that the group is composed of five objects, or that it is a fiveish group. But in saying these things we are not saying something new; we are not adding anything to what we already know.

Learning how to count is like learning how to knit. Both are taught in terms of performatory and repetitively applicable rules. Compare a knitting pattern (k5, p6, . . .) and the rules for counting. A knitting pattern is a recipe, a set of rules (though not injunctions; nobody has to follow the pattern, nobody has to count in this way), rules which take the form: if you do this and this you get this result. The result comes about because the rules are so constructed that it should come about. And by courtesy we call the finished article a *knitted* jumper; a *fiveish* group. The jumper was made by the techniques and procedures of knitting; the group was made (selected? singled-out?) by the techniques and procedures of controlled counting.

The number of a class, then, is given by the numeral which we say when we count and stop. It is not truly a property of the class (and certainly not of the objects) but a result which we get when we count correctly. We should not look for numbers either in the world of objects or in the world of abstractions but in the techniques and procedures which give the numerals a use. What is a number? A number is a numeral which is used in controlled counting—not just in counting (pointing and saying) and not just as part of the counting rhyme. By controlled counting we mean counting and stopping in accordance with the rules.

But we have not yet said what it means to say that two classes have the same number of members. These two groups:

x x x x

have the same number of members means: if you control-count the first group and then control-count the other, you find that you have to stop at the same place: 'two'. We thus see that a lot of classes are similar. But this is not where arithmetic begins (as it does for Russell); we began that long ago. The number of a class is not the class of all those classes that are similar to it. We already know what the number of a class is and we use this knowledge to discover that some classes have the same number; *i.e.* that they are similar.

If we think of numbers in this way we do not have to discover a group with n members for the number n to be meaningful. For numbers are not names of classes which stand as the meanings and have to be there for the number to mean If there were only ten individuals in the universe, the number eleven would have a meaning and would be different from the number ten. For we know how to count endlessly with only a finite number of objects, and that makes 'eleven' different from 'ten'; and we know how

'eleven' could be used as a number because we know how to control-count. That is, we know what the number eleven means; and what it means is different from what ten means. We could in fact give 'eleven' a use (and not merely know *how* to use it in case there were eleven objects). For we could count through the ten objects and then continue from the beginning with 'eleven', *then stop*. We have thus control-counted the *pointings*. In this way we could reach any assigned number. There is always a set of "objects" corresponding to every number if we control-count not the things before us but (in a systematic way) the pointings to them.

This seems to get us out of one of Russell's major difficulties. For if "a number is anything which is the number of some class" (p. 19), there has to be a class with n members for there to be a number n. This being so, "let us suppose that the total number of individuals in the universe were (say) 10; then there would be no class of 11 individuals, and the number 11 would be the null class. So would the number 12. Thus we should have $11 = 12$; therefore the successor of 10 would be the same as the successor of 11, although 10 would not be the same as 11. Thus we should have two different numbers with the same successor" (p. 24). Peano's third axiom therefore fails and has to be saved by postulating the axiom of infinity: "If n be any inductive cardinal number, there is at least one class of individuals having n terms" (p. 131).

But how exactly does it get us out of Russell's difficulty? Russell does not require the axiom of infinity for finite arithmetic. He too is prepared to accept that we can reach any assigned inductive cardinal even though there is only a finite number of individuals in the universe. For let the number of individuals in the universe be n, then the number of classes of such individuals is 2^n; and the number of classes of classes of individuals is 2^{2^n}; and so on. Hence we can establish the existence of a class of m "objects", where $m > n$, by counting not the individuals but the classes of individuals (since $2^n > n$) or the classes of classes of individuals (since $2^{2^n} > 2^n$) or, if necessary, the classes of classes of classes of individuals; and so on.[4] Of course we get into trouble if we lump together into one lot the individuals, and the classes of individuals, and the classes of classes of individuals; but there is no question of that at this stage. It is simply that we look around for the most appropriate things to count within a given type, and

[4] Notice that this suggestion brings counting back into the foundations of arithmetic since it is now being used to establish the existence of a number

the choice—whether classes or classes of classes (say)—depends only on how far we want to count. We do not have to lump together objects of a different type in order to reach a large number *m*, we only have to get far enough up the hierarchy before we begin counting so that we can be sure that the "objects" will not run out before we get to *m*. It is like counting the number of pointings instead of counting the individuals to which we point, since in counting the classes we are counting something different from the individuals; we are counting arrangements or selections of them. "In this way any assigned inductive cardinal can be made to find its place among numbers which are not null, merely by travelling along the hierarchy for a sufficient distance" (p. 133).

But if this is so; if Russell can reach *any* assigned inductive cardinal, independently of the axiom of infinity, by *constructing* "objects" to be counted, instead of discovering them, why does arithmetic break down for him? And how, if at all, does this procedure differ from that which enables us to count up to any inductive cardinal by counting, in a systematic way, the pointings-to the objects?

Arithmetic breaks down because if n is finite, so is 2^n, and so is 2^{2^n}, and so on. There is thus no infinite class of objects within any *one* type. Within any one type, the "objects" run out after a given power of 2 is reached and hence, for that type, the number after the given power of 2 designates the null class; and this will be so whatever type we choose. No matter how far we travel up the hierarchy before we begin counting, there will be a finite limit to the counting which can take place within the chosen type; hence a finite limit to the number series.[5] We could try to construct an infinite class by lumping together all "objects", but this fails for two reasons. First, because we then involve ourselves in contradictions which result from a confusion of types (p. 134 ff); and second, because we should then in any case be begging the question. For in order to establish the existence of an infinite set which consists of the n individuals together with the 2^n classes of them, and 2^{2^n} classes of classes of them, . . . , and so on, we have to assume the endlessness of the constructions: classes, classes of classes, classes of classes of classes, . . . We have to use the dots in one sequence in order to establish them in another.[6]

It is now easy to see the difference between counting abstrac-

[5] See *Principia Mathematica*, Vol. II, Section C, pp. 181-183.

[6] But perhaps this last is not fair comment. For if we give a recipe for *constructing* the objects of the hierarchy, to say that they might run out is to say that the recipe might fail. But how could it? (See below.)

tions which are constructed out of individuals and counting the pointings-to the individuals. For one thing, we avoid any type difficulties. However far we go, when we count the pointings-to, we do not have to move from one type to another; we are not involved in classes, or classes of classes, or any other collection in the hierarchy. Pointings-to all belong to the same type; and they never run out. They cannot run out because their endlessness is guaranteed by a repetitively applicable rule; and this is to say that, *in principle,* given only n objects we can construct a class of other "objects" which can be counted indefinitely. Practically, of course, we cannot go on indefinitely—which means that we do not in this way establish the existence of an infinite class if an infinite class is thought of as an endless collection laid out before us. But it is a mistake to think of infinite classes in this way. To do so is to think of them as if they were rather like finite classes, because their members are all there, but peculiar, because we can never put our fingers on the last one. What we can only mean, however, by saying that there *is* an infinite class, is that wherever we start and however far we count, there is always a next; the process need never stop. And of this we are assured if we have a rule which ensures it. Such a rule is a rule for counting the pointings-to. Hence we may say that we have an infinite class within one type—though the type is different from any that Russell considered. The class of pointings-to is another class of physical objects if we consider actions to be objects in some sense; certainly they are physical.

The difference, then, is this. Although Russell can reach any assigned inductive cardinal, he can only do so by shifting from one type to another as the need arises. To reach m, it may be sufficient to count the classes; but to reach (say) $m+w$ it may be that there are not enough classes and that it is necessary to move to classes of classes and begin counting all over again. In the case of pointings-to, however, any assigned inductive cardinal can be reached continuously—*i.e.* by a simple extension of the same process (by a further application of the same rule). In this way we are able to establish the existence of an infinite class—in the sense that there is always a next member—whereas Russell is not.

But why do we need to do this? Why do we want an infinite class? Russell needs an infinite class in order to establish the existence of transfinite numbers. For no meaning can be given to a number m unless it has first been shown that there *exists* a "given class" of m terms (since this enters essentially into the definition of m) and this is so whether m is finite or infinite. Hence the existence of an infinite class has to be established so

that it may function as the given class in the definition of an infinite number. If there is no such class, the infinite number has no reference. The axiom of infinity succeeds in establishing the existence of one infinite class, namely the class of natural numbers, 1, 2, 3, . . . , since it ensures that every natural number has a reference (hence a meaning) different from every other. That is, the class, 1, 2, 3, . . . , is endless and cannot collapse into a finite set provided that "if n is any inductive cardinal there is at least one class having n terms". Hence a rule which creates a meaning for every finite cardinal is in fact a rule which creates the existence of an infinite class (the natural numbers), and this in turn can be used to give a meaning to transfinite cardinals.

The suggestion, therefore, that we count pointings instead of objects, provides a way out for Russell since in this way an infinite set is created independently of the axiom of infinity. But it seems not to be relevant if we adopt a definition of number different from Russell's. If, as we have seen, we do not need to construct a class of m terms in order to make the number m meaningful, do we need an infinite set? Cannot we define transfinite numbers independently of the existence of a given set, as we did with finite numbers?

Perhaps; but if we are to have arithmetic at all, we must show that no two numbers have the same successor and hence that the set, 1, 2, 3, . . . , does not collapse into a finite set. We need to know that *this* set, at least, is infinite even if we do not need it in order to define transfinite numbers. Why? Because we need to know that there is no end to the *possibility* of controlled counting.

Throughout, this is the heart of Russell's worry. If we run out of *things to count* (*i.e.* if controlled counting has a limit), we cannot give a meaning to the next numeral, we cannot define the next number. And this means that the limitless possibility of controlled counting is being put at the base of arithmetic; it is this that the axiom of infinity ensures—in spite of what he says about counting presupposing number.

Put in this light the problem is not a real one for us. There can be no end to the possibility of controlled counting, since we have a set of rules for both counting and controlled counting (if we count the pointings) which are designed to be repetitively applicable independently of the number of objects in the world; and they cannot suddenly cease to be applicable. Provided we have the recipe we know how to make the cake even if the ingredients are not available, and our knowledge of number is this kind of how-knowledge. By putting rules at the base of arithmetic we represent it as a technique which *qua* technique

cannot fail. The rules are the guarantee of success; they have success built into them. Hence we need to postulate nothing more in order to ensure that the set of *numbers* 1, 2, 3, . . . , is infinite with every member different from every other. That characteristic of it is already there in the rules we have.

Or we may say: no two *numerals* have the same successor—the rules for the counting rhyme forbid it; and the counting rhyme, and counting, can go on endlessly. And no two *numbers* have the same successor; for numbers are simply numerals used for a certain purpose: a number is defined by the rules for the use of the numeral in controlled counting. Counting gives the numerals a use and controlled counting gives counting a use; that is, controlled counting further specifies the use of the numerals and so gives a meaning to number. This is *all* that a number is. There is thus no difficulty about the successor of any given number provided there is no difficulty about the successor of any given numeral.

But this still leaves us with the problem of defining the transfinite numbers. We have shown that there is a class with an infinite number of members, and for Russell that is enough. He can now define the number χ_0[*] as the number of members in the class, 1, 2, 3, . . . (or as the number of members in any similar progression). Thus "χ_0" is the answer to the question, "How many natural numbers are there?", as "5" might be the answer to the question, "How many crosses are there here: x x x x x?"

But there are difficulties in this. Consider again how it is that we get the idea of the number of individuals in a class. We count and *stop*. Thus counting and stopping is the procedure to be carried out in answer to the question "How many?". So if we ask how many numerals there are in the set, 1, 2, 3, . . . , it is clear that this is a different question. For essentially we cannot stop, otherwise we have only a finite number. And Russell would be the first to agree that χ_0 is not a number that we can ever count to; we should not get to it though we counted endlessly. In fact, to say that there is an χ_0-class is just to say that it is possible to go on counting endlessly; it is to say that we need never stop; it is to say that there is always a next number.

There is of course a difference between the questions: 'Is there always a next number in the set 1, 2, 3, . . . ?' and 'How many numbers are there in the set 1, 2, 3, . . . ?'. They are, however, related. If we answer yes to the first, either by postulating the axiom of infinity or in some other way, we are ensuring that there is no end to the possibility of *controlled*

[*] This symbol is to represent the Hebrew aleph with suffix zero as used in Russell's *Introduction to Mathematical Philosophy*.—Ed.

counting. This is so since the set is used for controlled counting, and the question, it should be noted, is 'Is there a next number . . . ?' not, 'Is there a next numeral . . . ?'. But this answer yes to the first question entails that the answer to the second question cannot be a finite number; that is, the set cannot be used to determine the number of members it itself has; and this means that it cannot be control-counted. Hence the number which is the answer to the second question (χ_0) is different from the finite numbers (those in the set) and since these, and only these, can be defined in terms of controlled counting, its definition has to be expressed in some other way. How then is it to be defined? In terms of the limitless possibility of counting, rather than controlled counting. For we can apply the endless techniques of counting to the set 1, 2, 3, . . . , (point and say, point and say) even though we cannot control-count it at all. In general, to say that there *is* an infinite set is to say that we know how to count; to say that there *is* a finite set is to say that we know how to control-count.

Hence, whereas the definition of finite number has to be expressed in terms of controlled counting, the definition of transfinite number has to be expressed in terms of counting only. And to say that there are such numbers is to say that we can give counting a use independently of controlled counting. But we have seen that this is so.

What is left, therefore, in the definition of a transfinite number is just the notion of similarity. For counting is simply one-one correlating numerals and pointings (this is different from control-counting the pointings in order to see how many have been made). And that is why we cannot answer the question "How many?" when asked of the set of natural numbers The notion of similarity *of itself* (or counting by itself) is never sufficient for us to be able to answer this question (we can pair off men with women without knowing how many of each there are). It enters into the procedures for answering the question but it does not exhaust them. We have to have a further rule: stop when the end is reached. That is, we have to control the counting. If we remove the rule for stopping the question cannot be answered. Or, we might say, it means something different. And we might recognize this by saying that χ_0 is a different kind of number from, say, 5. That is, we expect it to obey different laws.

But this means that the definition of a transfinite number differs essentially from the definition of a finite number; means, indeed, that they cannot be defined in the same sort of way. Yet Russell does define them in the same sort of way. Each is defined by reference to a given class and the notion of similarity. And this

makes him treat the question "How many" as if it were the same kind of question when asked of the two sets: x x x x x and x x x . . . But it cannot be, otherwise χ_0 would not be a different kind of number from 5; we should not have laws like: $n\chi_0 = \chi_0$ and $\chi_0{}^n = \chi_0$.

Perhaps this is being too naive. For it may well be said that the two definitions bear only a *prima facie* resemblance; in fact however they differ essentially because the given sets are essentially different. And we must not ignore the given sets; at no time does Russell rely on similarity alone. In the case of 5, the given set is finite; in the case of χ_0 not. But how do we know that they are different? By postulating the axiom of infinity (or by showing in some other way that there is an infinite class). And *that* is the need for the axiom. No; this will not do. What the axiom of infinity does is to establish that the class, 1, 2, 3, . . . , *cannot* be control-counted; it establishes that only counting is applicable here; it establishes that only similarity can be used. The axiom of infinity *removes the need* for the given class (for endless counting can take place with a finite class). Then *here* is the difference; for in the case of finite numbers the given class is essential. But why is it essential? Because it establishes the finitude. No; it does not.

There is nothing in Russell's *definition* of number which enables us to distinguish between the finite numbers and the transfinite numbers. We have to rely on some non-definitional criterion of finitude. We have somehow to know that 'x x' is a different sort of class from 'x x x . . .' Then what is the criterion? It is that we can exhaust the one by control-counting but that control-counting is inappropriate to the other. The possibility of stopping, of exhausting the class, is what makes it different from an infinite class. Stopping, therefore, as a procedure in controlled counting is not simply an alternative to the "given class". It adds something about what kind of class the given class is. And this is just what Russell's definition does not do. There is nothing in Russell's definition of finite number which takes the place of stopping; and it is not, therefore, a definition of *number*.

[1] Finite numbers satisfy Peano's inductive axiom whereas transfinite numbers do not; and Russell uses this axiom to define the inductive cardinals. Hence, it may be said, he is defining their finitude But this seems to confuse two separate bits of knowledge: (1) that they satisfy the inductive axiom and (2) that they are finite. For we could generate the inductive cardinals by means of the inductive axiom and yet not know that the numbers so defined are *finite* Similarly, we could define χ_0 axiomatically (i.e. implicitly in axioms) and still not know that it is *transfinite*. We should only know that the properties of the one differed from the properties of the other, but we should not know in what respect. Hence, knowing that the difference between them is a difference between finite and infinite comes from elsewhere. (See *Principia Mathematica*, Vol. II, Section C, * 120, p. 200 ff.)

All of Russell's troubles spring from the fact that he nowhere gives a recipe for finitude.[7] Hence when he wants to distinguish infinite classes from finite classes he has to find some further characteristic which infinite classes satisfy. He has to *add* something to the definition of number—the axiom of infinity. But this trouble does not arise if finitude is first specified and an infinite class is then defined by *removing* one of the characteristics which finite classes satisfy. Or, to put the same thing in another way: if an infinite process (counting) is first specified and finite classes are then defined by making *them* satisfy an additional characteristic (stopping). That is how we began. Counting, as an endless routine, comes first; counting the number of terms in a group is then achieved by controlling this endless process, by bringing it to an end. This is why we avoid the need for an axiom of infinity; its effect is secured by the repetitiveness of the counting rules.

Does this mean that we have an understanding of χ_0 before we have an understanding of 5? Yes; that is exactly what it means. But this should not be surprising; a repetitive technique is not difficult to understand. Knowing how to go on infinitely only involves knowing a finite set of rules; and repetitive rules which generate an infinite process can be understood independently of their finite application. There is nothing unusual in having a repetitive technique first before we control it. We have to know how to add one before we can add one to this particular number; we have to know how to screw (twist and push, twist and push, . . .) before we can screw in this particular screw; we have to know how to run before we can run to the shops.[8]

Then how do we define χ_0? We do not need to; we have defined counting. Or we can, if we please, define it as Russell did in terms of a one-one correspondence with the class 1, 2, 3, . . . ;

[7] One of the most confusing of all conceptual inversions is the belief that we get to know what infinity is by "extending" our notion of finitude (so that it is similar, but somehow different). Thus, "Every one that has any idea of any stated lengths of space, as a foot, finds that he can repeat that idea, and joining it to the former, make the idea of two feet; . . and so on, without ever coming to an end of his additions, and . . . he finds that . . . after he has . . . enlarged his ideas as much as he pleases, he has no more reason to stop, nor is one jot nearer the end of such addition than he was at first setting out. The power of enlarging his idea of space by further additions remaining still the same, he hence takes the idea of infinite space." (Locke, *Essay*, Bk. II, Ch. 17, 3.) But the truth surely is, as Leibniz says, that the infinite "is prior to all composition and is not formed by the addition of parts"; for the difference between infinite and finite is simply the difference between a technique and the application of it, and knowledge of the technique comes first. Perhaps Locke is half right since he does emphasise that our knowledge of infinity rests on knowledge of a technique (we find that we *"can repeat* that idea" e.g.), but he is half wrong since he thinks that our knowledge of infinity *arises from the application* of the repetitive technique, whereas it must precede the application.

for if we do this, we are saying less than we say when we define the number 5. We are relying on similarity only, hence on counting and not on controlled counting. Or, if we do not like it this way, if we feel uneasy about the "given class", then by stipulating that an χ_0-class is one which can be put into a one-one correspondence with a subclass of itself. Either of these ways is possible and either serves to distinguish transfinite numbers from finite, for in each case the definition is based on counting and not on controlled counting.

The number 0 raises similar difficulties. Why does it have properties like $n0 = 0$ and $0^n = 0$ if it can be defined in the same sort of way as the number 5? It is a different sort of number and so has to be defined in a different way. (In any case how do we one-one correlate the members of an empty class with the members of another empty class? There is no procedure to be followed, so the procedure of one-one correlating cannot *here* yield a definition.)

This problem does not arise if finite numbers are defined by counting and stopping. For since we cannot start, we cannot stop; and 0 is not therefore a finite number. How then do we define it? By seeing how the question "How many?" would be answered. "How many things are there on the table?" invites an application of the procedures for controlled counting: it implies the injunction "Control-count them!" But in two quite different contexts the command cannot be obeyed. The first context arises when counting cannot begin. And it is this which gives a meaning to 0; in such a context we say that there are no objects in reply to "How many?". We give an answer, but it is not the same sort of answer as "5". What we have said in fact is that the procedures cannot be applied, not that we have applied them and come up with a result. The second context arises when counting can begin but cannot end ("How many natural numbers are there?", "How many pointings-to are there in counting?"); that is, controlled counting is not applicable; the objects never run out. In this context we give the answer "χ_0", this time as an indication that the procedures can only be applied partially. 0 means: counting cannot begin. χ_0 means: counting cannot end. 5 means: counting can begin but ends with 'five'. Three different kinds of definition and therefore three different kinds of number.

* * *

I have said that we have, or can have, numbers, and therefore arithmetic, long before we have to introduce the notion of similar classes—though I do not want to deny that the class of pointings and the class of objects which together function in controlled counting are similar. I mean only that we do not require two

similar sets of *objects,* as Russell does, in order to understand what numbers are, and I want now to extend this by showing that other arithmetical concepts can be given a meaning in terms of controlled counting.

Thus we might teach a child what 'plus' and 'equals' mean in the following way:

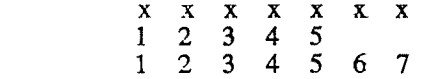
We might say: count (point and say) to four, and stop; then start counting again with the next object until you get to three. You have now counted four and three. This is what 'four plus three' means. And you can see that this is the same as seven. In this way: first mark the point you reached and then start at the beginning and count to seven. You can see that you get to the same point.



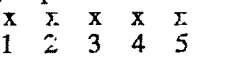
This is what 'equals' means. So you can see that four and three comes to the same point as seven; or, four plus three equals seven. (Is this an empirical fact or a logical one?) But 'four and three' does not mean the same as 'seven'. 'Four and three' means 'Control-count to four *and then* control-count to three', while 'seven' means 'control-count to seven'. And doing this we find that we get to the same point. We do two different things but get the same result; like two roads which lead to the same village.

'Greater than' and 'less than' might get a meaning in the following way:

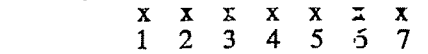
We might say to the child: in the counting rhyme, 'seven' comes after 'five'. So when you count to five and then stop, marking the place, and then start at the beginning again and count to seven and stop, you find that you have to go past the first point.



This is what 'five is less than seven' means and also what 'seven is greater than five' means. They mean the same. Why? Because the *same set of actions,* the same technique, *establishes both* statements. (Is this a logical fact or an empirical one?) We then show how to use this fact to determine the inequality of two groups. For since this group



contains five objects, and this group



contains seven objects, we say that the second group has a greater number of members than the first. Why? Because when we control-count the first we have to stop at 'five', and when we control-count the second we have to go past 'five', and 'seven' comes after 'five'.

Control-counting takes time; it is a temporal process. It is also a spatial process. That is why words like 'before', 'after', 'stop'; and phrases like 'comes to the same point as', are key words and phrases. They are logically prior to arithmetic in the sense that no child who did not understand them could do arithmetic. They are the logical primitives of arithmetic. Again, the 'and' in '2 and 2 make 4' is primarily the temporal 'and then' and only derivatively the connective 'and'. This is where the logical foundations are.

These kind of mathematical expressions,

$$2+3=5; \quad 7>5; \quad 5<7,$$

then, are *statements*. They are statements about what happens (or what will happen) when (or if) control-counting takes place. They are not rules, though they are true because of the counting rules—that is, the rules for saying the rhyme, the rules for pointing and the rules for stopping. And these rules are independent of the *kind* of objects counted. As any child can see, it makes no difference whether we count cows or crocodiles; it will still be true that four plus three equals seven. Why? Because as the examples have shown, we can teach the meaning of '4+3=7' even though there are only five objects; and this means that we are not truly counting the objects but the operations involved in counting—the pointings-to. But these are the same whatever the objects are—except that the objects must be distinct.

Each numeral in the counting rhyme is a different noise; it is distinct from all other numerals. And each pointing action in the counting process is distinct from all other pointing actions in that process. Since 'count' means 'point and say' therefore, it follows that the only kind of things that can be counted must be distinct. Hence, that the objects counted are to be distinct is a *condition* of being able to count. (The suggestion that arithmetic breaks down because two drops of water and two drops of water make one drop is therefore quite irrelevant.)

To have a different kind of arithmetic it follows that we must have a counting process which does not depend on distinctness. Either we must remove the distinctness of the numerals or the distinctness of the objects.

Suppose, first, that we remove the distinctness of the numerals. Suppose that the counting rhyme is 'one', 'two', 'one', 'two',

endlessly (so that, e.g., what we, using our arithmetic, would call the third noise, is not different from the first) while the pointing is as before. Then with the same sort of rule for controlled counting (if you have to stop at 'two' e.g. then that is the number of objects in the group), it follows that this group,

x x x x x x
1 2 1 2 1 2

contains two objects, while this,

x x x
1 2 1

contains one object; and so on. And we shall have $1 + 1 = 2$; since

x x
1 and then 1

comes to the same point as 1 2

But $1 + 1 + 1 = 1$ since,

x x x
1 and then 1 and then 1

comes to the same point as 1 2 1

Similarly, $2 + 1$ (1, 2, and then 1) will equal 1 (1, 2, 1); while $2 + 2$ will equal 2. 'Less than' and 'greater than', on the other hand, find no meaning since 'one' comes both before and after 'two'.

Such an arithmetic has an application, though a minimal one. It enables us to discriminate between odd and even groups. Thus,

x
x x x
x x x x x

} contain the same number of objects—one. (The groups are 1-1 correlated in terms of this arithmetic)

while,

x x
x x x x
x x x x x x

} each contain two objects.

Hence, this kind of counting is useful for some purposes; and if we belonged to a tribe which held oddness in great respect, it would be important.

Suppose, now, however, that the numerals in the counting rhyme are distinct but that the objects are not. We know how to say the numerals but we do not know how or where to point.

We might, for example, wish to count the number of points in the line,

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and to decide whether there are more or less points in the line,

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There are two things we can do. Either we can introduce artificially created distinct points and so invent a set of "objects" to be counted; or we can lay down some arbitrary criterion for 'the same number as'. In the first case we arithmetize the geometry; that is we force the techniques of arithmetic on this new situation. In the second case we geometrize the arithmetic; that is, we change the techniques by altering the criteria.

We arithmetize the geometry by introducing artificial units of length and we count these lengths; *i.e.* we *measure*. And we say that two lines which come to the same point (on the measuring rod) are equal in length. In this sense of 'point', points on the line and points on the measuring rod can be pointed to. Thus we are able to say that the first line above contains more points (units, objects) than, or is longer than, the second.

We geometrize the arithmetic, on the other hand, if we say that the two lines (unequal in length) contain the same number of points (objects) as each other. In this sense of 'point', points cannot be pointed to. And now we have to modify the arithmetic in such a way that it is based on a different kind of counting, for we cannot now point to the things counted. We have to construct a set of numerals which cannot be used for counting in the ordinary way (indeed which cannot themselves be counted) but nevertheless is such that there are as many numerals as there are points on the line. Such a set is postulated to be the numerals in the interval $(0, 1)$. There is no counting rhyme associated with these numerals. We do not know which numeral is next after '$\frac{1}{2}$' any more than we know which point comes next after an arbitrarily chosen one. We have a *free choice* of the numerals $>\frac{1}{2}$ and <1. This is what we mean by saying that the line contains an infinite set of points. We mean that a different sort of counting is involved; free-choice counting. And because of this we have a different kind of infinity from the χ_0-kind. An χ_0-set is one to which the ordinary procedures of counting can be applied (though it cannot be control-counted); it is a denumerable set. The set of points on a line, however, is one to which the ordinary counting procedures cannot be applied; it is a dense set.

Can we have a different arithmetic? Of course; geometry is a different arithmetic.

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COLLINGWOOD ON ART AS 'IMAGINATIVE EXPRESSION"*

By THEODORE MISCHEL

I

The central claim of Collingwood's *Principles of Art*[1] is that art is an activity in which we imaginatively express, and thus become aware of, our emotions. He holds that what expresses our emotions is a consciously controlled bodily activity, an activity which he calls "language" and identifies with art. But when looking at a painting or listening to a symphony we do not, in normal cases, perform any controlled bodily activity. Collingwood is, of course, aware of this, but contends that the controlled bodily gestures in which the audience expresses its emotions are made "in imagination". When we look at a Masaccio we may not actually move about in various ways, but we "imagine ourselves as moving in these ways". And similar considerations apply to music and the other arts.[2]

I will argue that this appeal to "imaginary gestures"—i.e., gestures which are not actually made, but are made "in one's head"—is incompatible with Collingwood's own account of imaginative expression. The notion that such imaginary gestures can be the "language" which expresses emotion rests, so I will argue, on a confusion of radically different senses of "imaginary". This confusion infects not only Collingwood's account of aesthetic experience, but also his account of the artist's experience, so that his whole discussion of art as "imaginative expression" involves serious internal incoherences.

To make good these claims I must begin by stating, in some detail, the salient features of Collingwood's "theory of imagination". Thereafter I will try to show that his account of art as imaginative expression conflicts fatally with that theory.

II

Collingwood contends that if we are to understand fully what is meant in saying that art is the imaginative expression of emotions, we must begin by examining the "general characteristics of experience as a whole", so as to make clear the place of imagination in the structure of experience.[3] This leads to a

* I acknowledge with gratitude a grant from the Lucius N. Littauer Foundation, Inc., which helped me to prepare this study.

[1] Oxford University Press, London, 1938. Galaxy Books, New York, 1958.

[2] *Principles*, pp. 147 and 243.

[3] *Principles*, pp. 151-153.

"theory of imagination" (Book II of the *Principles*), which is then used to "explain" the nature of art (Book III, "The Theory of Art").

Experience, we are told, presents us with "feelings", and "every feeling is both sensuous and emotional".[4] Further, there are two "stages" through which a feeling may pass: the "bare feeling, below the level of consciousness" on the one hand, and "a feeling of which we have become conscious" on the other.[5] Collingwood calls experience below the conscious level "psychical experience"; this is the evanescent, ever-shifting flux of sensuous-emotional experience which is given to "attention" as something to work on. Attention (or "consciousness") is the activity which discriminates "concrete individuals" from the flux of feeling, "stabilizes" them so that they can be held before the mind and compared with one another, and thus "dominates" and "domesticates" brute feelings.[6] He further holds that "mere feeling cannot even tell me what I now feel". For

> "If I try to fasten my attention on this present feeling, so as to give myself some account of its character, it has already changed before I can do so. If, to take the other alternative, I succeed in doing so . . . the feeling to which I attend must be somehow stabilized or perpetuated in order that I may study it, which means that it must cease to be mere feeling and enter upon a new stage of its existence".[7]

In other words, psychical experience is experience at a pre-conscious level—experience of which I am never aware but which is "presupposed" by my conscious experience—and when I attend to my experience, thus becoming conscious of certain feelings, these cease to be bare feelings and become something else: "from being *impressions of sense,* they thus become *ideas of imagination*".[8]

It is important to note that Collingwood here uses the term "imagination" for experience at the conscious level. Indeed, "regarded as names for a certain kind or level of experience, the words consciousness and imagination are synonymous".[9] The difference between them is that while "consciousness" refers to the

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[4] *Principles,* p. 221; see also pp. 160-164

[5] *Principles,* p. 213. Collingwood actually distinguishes three such stages, the third being a conscious feeling which "we have placed in its relation to others". However, for the purposes of this paper this third stage—what Collingwood calls the level of "intellect" (pp. 215 and 221)—need not be discussed

[6] *Principles,* pp. 203-211, 221-223.

[7] *Principles,* p. 222.

[8] *Principles,* p. 222; my italics.

[9] *Principles,* p. 215.

activity which dominates and domesticates bare feelings, "imagination" is the name for feelings which have undergone this process. "Imagination is thus the new form which feeling takes when transformed by the activity of consciousness".[10]

In short, Collingwood holds that the conscious (imaginative) level of experience presupposes a psychical level as its "foundation". He calls experiences at this pre-conscious level "impressions" ("brute feelings", "sensations"). And he claims that *consciousness* (attention) is the activity which dominates and discriminates such feelings, thus transforming them from mere *impressions* at the *psychical level* into *ideas* at the *imaginative level* of experience.

Since every feeling is said to have an emotional as well as a sensuous component, these two components will also be present in every idea. Further, Collingwood insists that "to whatever level of experience an emotion may belong, it cannot be felt without being expressed. There are no unexpressed emotions".[11] But the way in which emotions are expressed at different levels of experience differs. *Psychical expression,* Collingwood tells us, "consists in the doing of involuntary and perhaps even wholly unconscious bodily acts. . . . Thus, certain distortions of the face express pain".[12] But at the imaginative level emotions receive a different kind of expression. Just as the work of consciousness transforms brute feelings (impressions) into ideas, so it performs a similar transformation in

> "the bodily act of expression, raising it from the crudely psychical level to the imaginative.
>
> The general nature of this change can be expressed by saying that just as our emotions no longer arise in us as brute facts, but are now dominated . . . so the bodily acts which express those emotions, instead of being simply automatisms of our psycho-physical organism, are experienced in our new self-consciousness as activities belonging to ourselves, and controlled in the same sense as the emotions they express".[13]

Such consciously controlled bodily acts expressing emotion are called *imaginative expression.* While the emotional component of an impression requires psychical expression, the emotional component of an idea requires imaginative expression. And since there are no unexpressed emotions, the occurrence of an emotion at any level of experience is always correlative with its appropriate expression at that level.

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[10] *Principles,* p 215.
[11] *Principles,* p. 238.
[12] *Principles,* p. 229.
[13] *Principles,* p. 235.

Collingwood calls the imaginative expression of emotions *language* in its primitive or original form. "Language", we are told, "is simply bodily expression of emotions dominated by . . . consciousness".[14] He thus uses the word "language" in a very wide sense to refer to "any activity of any organ which is expressive in the same way in which speech is expressive", where "speech" is taken to mean not words and sentences, but a "system of gestures made with the lungs and larynx".[15] Any other system of consciously controlled gestures constitutes another kind of language. Moreover, "every kind of language is in this way a specialized form of bodily gesture, and in this sense it may be said that the dance is the mother of all languages".[16]

Collingwood then identifies art with language. "The aesthetic experience, or artistic activity, is the experience of expressing one's emotions; and that which expresses them is the total imaginative activity called indifferently language or art".[17] Indeed, the "theory of art" which is developed in Book III of the *Principles* purports to be simply an elaboration of the consequences which follow from this identification. Finally, Collingwood regards artistic and aesthetic experience as substantially identical: in so far as the audience understands a work of art it "repeats" the artist's creative experience.[18] The physical artifact is merely a record left by the artist's "speech-gestures" and an indication to the audience of the speech-gestures which it must perform in order to reconstruct the artist's experience.[19] Thus "the written or printed book is only a series of hints, as elliptical as the neumes of Byzantine music, from which the reader thus works out for himself the speech-gestures which alone have the gift of expression".[20] Unless the audience takes the "hints" provided by the artifact, and by means of them repeats the artist's speech-gestures, it can have no aesthetic experience. For without these gestures there is no language, and thus no art.

III

How then is it possible for the audience to have an aesthetic experience unless it performs all sorts of bodily gestures which

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[14] *Principles*, p. 235.

[15] *Principles*, p. 243. Collingwood distinguishes such primitive language from "intellectual language" (or "symbolism"), though he claims that the latter is "the same thing intellectualized, or modified to express thought" (p. 225) His discussion of how language is "intellectualized" is beyond the scope of this paper.

[16] *Principles*, pp. 243-44.

[17] *Principles*, p. 275.

[18] *Principles*, pp. 118-119, 149, 308.

[19] *Principles*, pp. 247-252.

[20] *Principles*, p. 243.

are not in fact performed? Collingwood replies that the audience performs these gestures "in imagination". Though we sit still at a concert, we imagine ourselves as moving in all sorts of ways while we listen to the music, and these imaginary gestures constitute our activity of "imaginative expression". Cr "when looking at a Masaccio we need not walk straight through the picture, or even stride about the gallery; what we are doing is to imagine ourselves as moving in these ways".[21]

But a gesture made in imagination, in this sense, is not a gesture made at all—it is only the idea of making a gesture; that is, it is an *idea* rather than a *bodily activity*. And while ordinary language contrasts an "imaginary activity" with a bodily activity which is actually performed, this is simply not what that phrase means in Collingwood's theory of imagination. For, according to that theory, when consciousness raises a bodily act from the psychical to the imaginative level, that act ceases to be automatic and becomes instead a *consciously controlled bodily act*.[22] Just as an idea is an impression which consciousness has raised from the psychical to the imaginative level, so an act of imaginative expression is an automatic bodily act (i.e., "physical expression") which has undergone an analogous transformation. "Materially", Collingwood tells us, "this act [i.e., act of imaginative expression] does not differ at all from that of psychical expression". For it differs from psychical expression "not in its own intrinsic nature, but in its relation to the general structure of experience", so that "an expressive act may be an act of precisely the same kind, whether its expressiveness is psychical or imaginative".[23] And Collingwood explicitly identifies language with the consciously controlled bodily gestures which express emotion at the imaginative level of experience.[24] At this level, what is expressed is an idea, and what does the expressing is a consciously controlled bodily act—not another idea.

If now we go on to say—as Collingwood does—that "by creating for ourselves an imaginary experience or activity, we express our emotions; and this is what we call art",[25] then the only thing that can *properly* be meant by calling these controlled bodily gestures an "imaginary activity" is that it is an activity which occurs at the level of imagination or consciousness; in other words, that it is "language"—i.e., a consciously controlled bodily

[21] *Principles*, p. 147. See also p. 243.
[22] See the quotation from p. 235 of the *Principles* cited earlier (footnote 13).
[23] *Principles*, p. 236. Collingwood claims that the difference between the child's "automatic cry" of hunger, and the "self-conscious cry which seems . . . deliberately altered in order to call attention to its needs" illustrates the difference between psychical and imaginative expression.
[24] *Principles*, pp. 235-236, 241-247, and passim.
[25] *Principles*, p. 151.

activity. It cannot mean, in terms of the theory of imagination, that this bodily activity can be "imaginary" in the sense of not actually performed. This would be self-contradictory since Collingwood explicitly claims that "imaginative expression" is "language", and that "language is simply bodily expression of emotion, dominated by thought in its primitive form as consciousness".[26]

But this is precisely the point which Collingwood muddles when he appeals to the performance of "imaginary gestures" on the part of the audience. For now his point is that when the spectator appreciates a painting his experiences are "not actual motor sensations, they are imaginary motor sensations".[27] Or again, he speaks in another place of "real or imaginary movements, as of dancing, in the audience".[28] But when "imaginary" is thus contrasted to "real", or "actual", it means something totally different from what it means in Collingwood's theory of imagination. The point now is not that these gestures are consciously performed, but that they are not performed at all—i.e., they are performed "in imagination", but not in actuality.

Now I may, indeed, imagine myself performing all sorts of gestures when I stand before the canvas. I can "sing mentally", and "in imagination" I can try to follow the movements of the dance. But the point is that these are not "imaginary gestures" in the sense in which Collingwood's theory of imagination requires that there be "imaginary gestures" for expressing emotions at the imaginative level of experience.

Given Collingwood's theory of language, the idea of a bodily gesture (i.e., a gesture "made in one's head") cannot ever express emotions. For he claims that

> "when language is said to express emotion, this means that there is a single experience which has two elements in it. First, there is . . . an emotion of which the person who has it is conscious, and which by his consciousness he has converted from impression into idea. Secondly, there is a controlled bodily action in which he expresses this idea. The expression is not an afterthought to the idea; the two are inseparably united, so that the idea is had as an idea only in so far as it is expressed."[29]

Thus it is the controlled bodily activity which expresses, and the idea which is expressed. Since a gesture made in one's head is itself an idea—i.e., an imaginative (conscious) experience having an emotional as well as a sensuous component—it could never serve to express another idea. Indeed, the idea of a bodily activity

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[26] *Principles*, p 235.
[27] *Principles*, p 147.
[28] *Principles*, p 243.
[29] *Principles*, p. 249.

could itself be "had as an idea only in so far as it is expressed" by some controlled bodily activity.[30]

But Collingwood's ambiguous use of "imagination" makes it easy for him to miss this. Since the controlled bodily gestures which he identifies with language, or art, occur at what he has called the "imaginative" (conscious) level of experience, he refers to them as an "imaginary activity". Having done this, he sees no difficulty in claiming that the "language" which is the *sine qua non* of the spectator's aesthetic experience consists of "imaginary gestures". Thus he slips from the special sense which his theory has given to "imagination" to what that term ordinarily means.

IV

What misleads Collingwood—not only in his discussion of aesthetic experience but also in his account of the artist's experience—is, I believe, his idealistic bias against regarding art as physical. Like Croce, he wants to show that art is not a "bodily thing" but is essentially "imagination". But at the same time he is also committed to the view that the language which expresses our emotions is always a bodily activity—it is "speech-gestures which alone have the gift of expression". Thus while the theory of language which Collingwood develops in Book II leads one to *expect* that the gestures which the artist makes in painting must be the "language" which expresses his emotions—the language which Collingwood identifies with art—Book III explicitly denies this. Instead, Collingwood agrees with Croce not only in insisting that the painted picture is not the work of art (in what he claims to be the "proper" sense of that phrase), but also in holding that the painter's artistic activity ("language", "imaginative expression") is *not* to be identified with the physical activity of painting. As Collingwood puts it:

> What has been asserted is not that the painting is a work of art, which would be as much as to say that the artist's aesthetic activity is identical with painting it; but that its production is somehow necessarily connected with the aesthetic activity, that is, with the creation of the imaginative experience which is the work of art.[31]

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[30] Strictly speaking, it is, of course, the emotional component of the idea which requires expression in a controlled bodily activity.

[31] *Principles*, p. 305. See also pp. 303-305. It is worth noting that Collingwood here differs from Croce only on the question of "externalization" For Croce the manipulation of the physical medium is a purely non-aesthetic activity which the artist may perform for the sake of permanence and communication; such physical externalization has nothing to do with his aesthetic experience But Collingwood explicitly criticizes the doctrine of externalization (*Principles*, pp. 301-302, 304-305) and insists that the artist's aesthetic experience is "somehow necessarily connected" with the production of the physical artifact.

How are we to understand the claim that the artist's physical manipulation of the medium is different from, though "necessarily connected" with, his aesthetic activity? Collingwood's answer is that while the artist's aesthetic experience is "purely imaginative", still

> every imaginative experience is a sensuous experience raised to the imaginative level by an act of consciousness . . . the aesthetic experience is an imaginative experience. It is wholly and entirely imaginative. . . But it is not generated out of nothing. Being an imaginative experience, it presupposes a corresponding sensuous experience.[32]

This "sensuous experience" is

> the artist's psycho-physical activity of painting; his visual sensation of the colours and shapes of his subject, his felt gestures as he manipulates his brush . . . in short, the total sensuous (or rather, sensuous-emotional) experience of a man at work before his easel. Unless this sensuous experience were actually present, there would be nothing out of which consciousness could generate the aesthetic experience. . . . But this sensuous experience, although it is actually present, is never present by itself. Every element in it comes into existence under the eyes of the painter's consciousness . . . and every element in it is therefore converted into imaginative experience at birth.[33]

Thus Collingwood claims that while the artist's imaginative (aesthetic) activity is not to be identified with the physical manipulation of the medium, it is, nevertheless, inseparable from it because the latter is the psychical ("psycho-physical", "sensuous-emotional") experience which his imaginative experience presupposes.

But this is very queer. How can the artist's "felt gestures as he manipulates his brush" be part of his psychical experience? Surely these gestures are not "involuntary and perhaps even wholly unconscious bodily acts"—i.e., instances of what Collingwood has called "psychical expression". Since the gestures made in painting are consciously controlled, how can they belong to the psychical rather than the conscious level? And what is even more puzzling, if the gestures made in painting are not the controlled bodily gestures which express emotion at the imaginative level, then just what *are* the controlled bodily gestures which Collingwood takes to be "language" or "art"? After all, when a painter is at work before his easel no other gestures—not even implicit ones—seem to be involved. To say, as Collingwood does, that

[32] *Principles*, p. 306.
[33] *Principles*, p. 307.

every element in the psycho-physical activity of painting comes into being under the eyes of the painter's consciousness and is, therefore, "converted into imaginative experience at birth" does not clarify anything. For while it is clear what it means to say that the painter's "visual sensations" are transformed into imaginative experience at birth—i.e., they become ideas—it is by no means clear what it means to say this about the artist's "felt gestures as he manipulates his brush". Since they *are* controlled bodily gestures, they are already at the imaginative level—this is just why one is puzzled to find them included in the artist's "psycho-physical activity". And if Collingwood now wants to say that these "felt gestures" are transformed at birth into something else, then just what are they transformed into?

The only answer I can suggest is that Collingwood's idealistic bias leads him to confuse radically different senses of "imaginary". Since he identifies art with language, and since he is "using the word 'language' to signify any controlled and expressive bodily activity, no matter what part of the body is involved",[84] the controlled bodily gestures made by a man at work before his easel *must* constitute the activity which Collingwood calls "language" or "art". For the painter makes no consciously controlled bodily gestures other than the ones involved in painting. But Collingwood wants to show that Croce is right in regarding art as essentially "imagination" and not a "bodily thing". And he has called the controlled bodily gestures which express emotion at the conscious level an "imaginary activity", a phrase which ordinarily denotes an activity which is not actually performed. This makes it easy for him to think that the artist's "imaginative expression" ("language") must somehow differ from the merely physical activity of painting; it must be that activity transformed by consciousness from something merely physical into something "imaginary". He is thus led to confuse "imagination" in the sense in which his theory defines it—i.e., conscious as opposed to psychical—with the ordinary sense of "imagination", the sense in which it is opposed to what is physically real ("bodily things").

If this confusion is avoided, then it follows from Collingwood's own account of language (imaginative expression) that: (1) the controlled bodily gestures made by the artist in manipulating his physical medium are identical with his artistic activity; (2) it must, therefore, be impossible ever to produce a work of art in one's head; (3) it is impossible for the audience to have an aesthetic experience unless it performs some sort of controlled bodily activity. But Collingwood himself explicitly denies all of the foregoing conclusions. Moreover, since it is possible to com-

[84] *Principles*, p. 241. See also p. 235.

pose a short poem in one's head, and to have an aesthetic experience without making any bodily motions, (2) and (3) are clearly contrary to fact.

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NOWELL-SMITH'S *ETHICS*

By H. J. McCLOSKEY

There is little doubt that Nowell-Smith's *Ethics* must be ranked as one of the more significant contributions to ethical theory in this century. However, illuminating as is so much of the discussion of the uses of normative and kindred words, and impressive as are many of his criticisms of ethical objectivism, Nowell-Smith fails in his main purpose—namely, of offering a defensible substitute for traditional ethical objectivism. In respect of the big problems for subjectivism—the problem of the rationality of morality as it arises in explaining personal perplexity and deliberation, inter-personal advice, and moral disagreement; the problem of the authority of morality; the problem of characterising the moral and discriminating it from the non-moral; and traditional problems of a like kind for subjectivism—Nowell-Smith's theory would seem to be just as exposed to the traditional difficulties as are those of Hare, Stevenson and other subjectivist predecessors. On certain issues Nowell-Smith's account is no doubt more plausible than these earlier theories; but overall it would seem to be just as exposed to most if not to all of the traditional, standard objections, and, in addition, to special difficulties of its own. It is this that I shall endeavour to bring out in this paper.

Nowell-Smith's *Ethics* arises out of his dissatisfaction with all ethical theories of the past and present—subjectivist and objectivist alike. Both types of theories, he suggests, rest on a recognition of important features of moral discourse; but both, he claims, misrepresent those features the other type of theory stresses. However, although dissatisfied with the subjectivist analyses such as those of Stevenson and Hare, and although he is severely critical of the traditional forms of ethical naturalism, Nowell-Smith himself nevertheless looks to a subjectivist type of account of morality, which he then associates loosely with a position sympathetic to traditional naturalism. His subjectivist standpoint is clearly evident from statements such as: "Without pro- attitudes we should neither have any use for standards nor even be able to understand what a standard was."[1] But by contrast with and in opposition to most earlier versions of subjectivism and naturalism, he rejects the view that ethical terms can be *defined* subjectively or naturalistically, and explains them as Janus or multi- function words, and such that they can be explained but not defined by reference to their functions, their functions involving reference to pro- attitudes, the more ultimate of which are thought to be natural. In this way,

[1] *Ethics*, p. 181.

whilst rejecting traditional naturalism and subjectivism, Nowell-Smith seeks to retain what is the essential inspiration of each. He himself represents his conclusions as being partly the result of seeing what he regards as a number of fatal objections to objectivism (to 'intuitionism' and to what he equally misleadingly refers to as 'teleological ethics'), and partly as a result of a more positive approach to the study of the logic of moral expressions.

I shall not be concerned here to attempt to consider and assess Nowell-Smith's criticisms of ethical objectivism. It is sufficient for our purposes to note that one main line of criticism is to the effect that ethical objectivism springs from the allegedly mistaken label-bearer theory of meaning, and that, in rejecting the one, Nowell-Smith rejects the other and proceeds to develop an account of the function of moral expressions in terms of the Wittgensteinian theory of meaning. It is this account of meaning, rather than his other premisses or presuppositions, Nowell-Smith claims, which leads to his conclusions. He allows and even stresses that many of the facts that objectivism sought to explain, need to be explained—e.g., the nature of reasons in ethics, and the possibility of deliberation, advice and instruction in morals; the appropriateness of descriptions of moral expressions as true or false; the point of having moral principles, and of morality itself; the characterising of morality; etc. However, although he allows that these are phenomena which need to be explained, Nowell-Smith finds their explanation, not in a modified form of ethical objectivism, but in terms of a subtle, sophisticated version of ethical subjectivism. That it is a variety of subjectivism, and not a synthesis of objectivism and subjectivism, will not be argued at this point, but will become apparent in the light of the criticisms to be urged against it. However, it is sufficient here to justify this label—if it is thought to need justification—to refer to the statement already quoted, and to observe, as that statement suggests, that the essential theme of Nowell-Smith's work is that value words are to be explained in terms of pro- and con- attitudes, not in the sense of simply being defined in terms of them, but by being related to them in various ways, some direct, some less direct, more complex and devious.

Nowell-Smith's approach is explained in various ways, of which the following expositions are particularly revealing. He writes:

> "I have throughout treated 'What shall I do?' as the fundamental question of ethics and tried to show that moral concepts can only be understood by relating them, often in very indirect and complicated ways, to the concept of decision."[2]

[2] *Ibid*, p. 267.

This approach, combined with his constant concern to avoid the error he attributes to objectivism—of leaving a gap between the goodness of a course of conduct, i.e., its possession of a property, and our obligation to adopt it—involves the association of the subjectivist's emphasis on attitudes with the naturalist's stress on the relationship of good and natural human wants.

The other illuminating statement to be noted here explains Nowell-Smith's conception of the job to be done, and his manner of approaching it. He writes:

> "The old model is not just misleading; it is wholly wrong. The words with which moral philosophers have especially to do, which are usually called 'value-words', play many different parts. They are used to express tastes and preferences, to express decisions and choices, to criticise, grade and evaluate, to advise, admonish, warn, persuade and dissuade, to praise, encourage and reprove, to promulgate and draw attention to rules; and doubtless for other purposes, too. These activities form the complex web of moral discourse, and our problem is to trace the connexions between them and to come to understand how it is that the same word can be used in all these different ways. What a man is doing with a particular word at a particular time can only be discovered by examining what he says in its context, but it would be just as absurd to suppose that there was no connexion between these activities as to suppose that the same expression can only be used to do one job."[3]

This is the key to Nowell-Smith's whole account. Moral words are explained as multi-function words, serving many functions at any one time, and different sorts of functions at different times. Nowell-Smith's own term, 'Janus words', fails to carry the full implication of the range of complexity and variety of functions he attributes to moral terms; but then no one label would succeed in doing this.

Nowell-Smith develops this multi-function account of value words by reference to his distinction of three types of words (D—for descriptive words, A—for aptness words, and G—for gerundive words), or, rather, of the three uses of words in sentences, to his account of pro- and con- attitudes and words, and of the relations between the so-called aptness and gerundive words and pro- and con- words and their bases, pro- and con- attitudes, and also to the naturalness of certain basic pro- and con- attitudes.

To enlarge on this. Words have many uses; and the one word may be used in many ways. An understanding of the variety of uses of different, and of the same word/s is therefore claimed to be

[3] *Ibid*, p. 98.

necessary for an understanding of the complex functions of moral words. Hence the three important uses of words and of types of words are distinguished, Nowell-Smith being careful to stress that the same word may function at different times as any one of the three—D, A or G words. D words obviously need little explanation. They are simply words with a straight descriptive meaning, i.e., with connotation in the old sense of the term. A words—e.g., terrifying, hair-raising, disappointing—are explained as those which indicate that an object has certain properties which are apt to arouse a certain emotion or range of emotions. And G words—e.g., praiseworthy, noteworthy, laudable, damnable—the key words of Nowell-Smith's account, are only sketchily explained, and then largely by analogy with A words, as being less—and in a sense, more—explanatory than A words, as having different but overlapping and similar contextual implications, and as being similar to A words in being without ontological commitments of the kind involving unanalysable properties. The distinctive feature of G words, by contrast with A words, is said to be the fact that they imply that the relevant person not merely is likely to have a certain reaction, but that he ought to have it. However, it is A words, or rather the A uses of words, that Nowell-Smith discusses most fully, even though they are not typical normative words, but rather something intermediate between the fully evaluative and non-evaluative G and D words.

A words, he argues, are obviously words which do not function by standing for objective qualities; rather, they are words, the functioning of which is to be explained in terms of their having four elements—a subjective element, a predictive element, a generalizing element and a causal element. Thus when it is said that a scene is sublime, a subjective report and a prediction are being given, the latter on the basis of certain objective causal facts, and on the basis of the speaker's reaction, and at the same time a generalization is being implied about how other people react. These various elements are spoken of as the contextual implications, as is the assumption commonly made by hearers of assertions, that the person making the statement believes the sentence he asserts to be true. The confused nature of the concept of 'contextual implication' is apparent from this alone, for clearly some of these elements would qualify as part of the meaning of the assertion, whilst other so-called contextual implications are 'implications' of the purport of the sentence, whilst others again are 'implications' of the fact that it is used. Stevenson's concept of 'cognitive suggestions', just because it does not claim to have the logical rigour and status of Nowell-Smith's 'contextual implications', would seem to be a more accurate and defensible concept. But the confusions inherent in Nowell-Smith's use of the

expression 'contextual implication' may more usefully be considered elsewhere.

Aptness words are thought to provide the clue to a non-objectivist account of G and hence of moral terms; but, as already indicated, a clear and systematic account of G words is not worked out, nor is a criterion or a set of criteria for G words given. We are told that G words relate more obviously to choice, that G sentences are always explicitly for or against something, and that they relate to redirection of attitudes. They indicate how people ought to react, not how they will react.

The point of the discussion of G words is, of course, to bring out against the ethical objectivist that the use of a large range of value words, including names of the various virtues, can and needs to be explained without postulating special qualities. Yet when we come to consider how successful or otherwise Nowell-Smith is in meeting the standard objections to ethical subjectivism, it would appear that his account of G words contributes more towards supporting his criticisms of traditional ethical objectivist ontology than towards protecting his own positive contributions from the standard objections to subjectivism.

Although he discusses G words, and hence words relating to the virtues, such as 'trustworthy', Nowell-Smith discusses the moral uses of the more central moral expressions very little. He speaks of key moral terms such as 'good', 'right' and 'ought' as Janus words, and treats his discussions of A and G words as throwing almost sufficient light on the moral uses of these terms, even though he notes various differences between their uses and the characteristic uses of G words. It is, of course, true that Nowell-Smith does discuss uses of 'good' and of 'ought'; but even here it is the non-moral uses of these terms that are singled out for most attention.

As has been indicated, the concepts of contextual implication and of logical oddness are vital to Nowell-Smith's account of the logic of these expressions, for it is in terms of the contextual implications of the use of A and G words that he seeks on the one hand to avoid postulating unanalysable properties and on the other to explain the uses and explanations of and reasons for value judgments; whilst it is in terms of what he calls 'logical oddness' that he seems to seek to make explanation rather than justification of moral judgments central. Let us consider these concepts briefly. The concept of contextual implication is explained as follows:

> "For the concept of logical implication or analytic connexion between statements I propose to substitute the concept of 'contextual implication', and for the concept of self-contradiction that of logical oddness; . . .

> "I shall say that a statement *p* contextually implies a statement *q* if anyone who knew the normal conventions of the language would be entitled to infer *q* from *p in the context in which they occur*. Logical implications are a sub-class of contextual implications, since if *p* logically implies *q*, we are entitled to infer *q* from *p* in any context whatever. Contextual implication can be most easily illustrated in a case where there is clearly no logical implication.
>
> "If Jones says 'It is raining', Smith is entitled to infer that Jones believes that it is raining, although 'Jones believes that it is raining' clearly does not follow logically from 'It is raining'. Conversely, there is no contradiction between 'It is raining' and 'Jones believes that it isn't raining'; yet it would be logically odd for Jones to say 'It is raining, but I don't believe it is'."[4]

Nowell-Smith proceeds to give rules for contextual implication, explaining at the same time that they are not rigid rules. The rules given are three—that it is contextually implied that the user believes the sentence used to make a statement to be true; that the speaker contextually implies that he has what he himself believes to be good reasons for his statement; and that what a speaker says may be assumed to be relevant to the interests of his audience.

This account of 'contextual implication' is obviously unsatisfactory. The concept is far too vaguely defined—consider the vagueness of the key words in the above passage—"would be entitled to infer". By what right would they be entitled to infer? A discovery about a new type of legitimate inference should set out the nature of the inference and the grounds of its legitimacy. Instead, Nowell-Smith confers the honorific title of 'implication' on commonsense assumptions commonly made without error by ordinary men, without examining the grounds and justification of these assumptions. The 'entitlement' to make inferences seems to amount to no more than the fact that they are commonly made. It is little wonder, therefore, that as the book progresses the concept of contextual implication is applied with greater and greater abandon.[5]

The above quotation also introduces the concept of 'logical oddness', but this concept is more fully explained elsewhere as follows:

> "A question is logically odd if there appears no further room for it in its context because it has already been

[4] *Ibid.*, pp. 80-1.
[5] *Ibid.*, p. 115.

answered. This is not to say that the question is necessarily senseless, but that we should be puzzled to know what it meant and should have to give it some unusual interpretation.

"The task of the moral philosopher is to map the mutual relationships of moral words, sentences and arguments; and this is a task, not of showing how one statement entails or contradicts another, but of showing that in a certain context it would be logically odd to assert one thing and deny another or to ask a particular question."[6]

Much the same objections can be urged against this concept as against that of contextual implication in terms of its vagueness, lack of justification, and indiscriminate collecting together of many different sorts of things under the one label. But the difficulties raised by the concept are sufficiently evident without further discussion.

It is the reference to pro- and con- attitudes which give these concepts importance in Nowell-Smith's account of value words, for the contextual implications of such words are said to involve some sort of reference to pro- or con- attitudes, and hence to render many questions—particularly requests for justification—logically odd. Hence it is, too, that we are told that sentences containing pro- and con- words provide good, that is to say, logically impeccable explanations of choice. "They provide logically impeccable explanations of why someone chose to do something".[7] However, the account of what constitutes a pro- or con-attitude makes this assertion empty or circular, for, Nowell-Smith observes: "My lists of pro- and con- attitudes must be so construed that anything which could be offered as a logically good reason for or against doing anything must be included in the lists."[8] But how we are to determine what should count as a logically good reason for or against doing anything is not explained. In view of the key role played by these concepts in Nowell-Smith's account, it is a serious shortcoming that the problem of what is a pro- attitude is dismissed so cavalierly.

All these terms and concepts are used in Nowell-Smith's positive account; but they are, I should suggest, much more essential to its preamble than to its final form. In its final form the sort of thing he is putting up seems to be fairly simple. 'Good' and 'ought' sentences are explained as impersonal versions of sentences expressing pro-attitudes; they are impersonal in the sense that they contextually imply that they are based on reasons, the reasons including reference to general rules—standards, criteria, principles—and

[6] *Ibid.*, p. 83.
[7] *Ibid.*, p. 113.
[8] *Ibid.*, p. 114.

thence to pro- attitudes, the ultimate pro-attitudes being thought more usually to be natural pro- attitudes.

Nowell-Smith's account is, of course, much more complex than this. Both 'ought' and 'good' are shown to have many and varied uses. Depending, for instance, on whether the sentence containing the word 'good' occurs in the context of choice, advice, praising, commending, appraising, etc., different sorts of reasons, standards, or criteria, etc., will be contextually implied. But in the last instance they will relate, we are told, in some way to pro-attitudes. Appraisal, for instance, involves relating to usual standards, whilst moral appraisal is represented as consisting in determining which standards will bring about best what you 'prefer'; whilst moral deliberation about one's duties or moral principles is said to consist in thinking out what you really want—all this being heavily qualified in various ways. Thus, summing up his position, Nowell-Smith writes:

> "Moral philosophy is a practical science; its aim is to answer questions in the form 'What shall I do?' But no general answer can be given to this type of question. The most a moral philosopher can do is to paint a picture of various types of life in the manner of Plato and ask which type of life you really want to lead. But this is a dangerous task to undertake. For the type of life you most want will depend on the sort of man you are."[9]

And again:

> "The questions 'What shall I do?' and 'What moral principles should I adopt?' must be answered by each man for himself; that at least is part of the connotation of the word 'moral'."[10]

Let us look then at how Nowell-Smith's theory stands up to the traditional type of objections to ethical subjectivism and see whether, as I claimed, it is an account which amounts to little more than a cleverly and heavily camouflaged version of the unplausible, crude theory suggested by the passages just quoted.

II

When we look at Nowell-Smith's discussion closely, the thing that strikes us most forcibly is how little of the constructive discussion of 'good', 'best', and 'ought' is specifically of the moral uses of these terms. It is scarcely overstating the case to say that his account of the logic of 'good' in non-objectivist terms is too good; it is so concerned with explaining the variety of uses of 'good' in non-objectivist terms that the specifically moral uses of

[9] *Ibid.*, p. 319.
[10] *Ibid.*, p. 320.

'good' seem to become overlooked and left without characterisations.

The accounts of A and G words, by bringing out the 'logical oddness' of certain types of questions, and the nature of the various types of 'contextual implications' are thought to bring out the manner of functioning of 'good'. Nowell-Smith distinguishes various uses of 'good'—its use in the context of choice, advice, praising, etc., and in all these cases he makes a great play on the contextual implications of each use of the word, in each case reference being made at some point to pro- attitudes. However, when we look at the sort of examples with which Nowell-Smith is dealing, they are almost invariably non-moral cases of choice, advice, praise, etc. This is surprising in that the job he sets out to do, and the job of the moral philosopher, is, among other things, to explain and characterise 'good' in its moral uses, and to show how its applications in particular contexts of a moral character may be justified. When it is seen that Nowell-Smith's explanations fit non-moral uses, it becomes apparent that unless something significant is or can be added, they cannot be construed as giving an adequate account of the moral uses of 'good'.

This may be brought out by looking at his discussions of choice, advice and appraisal, and at how it is that he seeks to explain reasons in support of 'good' sentences in terms of his theory of contextual implication. Thus, when discussing 'good' in the context of choice, he argues:

> "The answer, 'Because it is best' . . . does not just imply a pro- attitude; it expresses it; . . . (and) . . . it contextually implies that I have reasons for my choice".[11]

That is to say, 'good' in the context of choice contextually implies that there are reasons, and even some of these reasons, although less in this latter regard than do G words, which in turn offer fewer reasons than A words; and it also expresses a pro- attitude.

Again, in the context of advice, he argues:

> "The considerations that apply to 'good' in the context of choice apply equally in the context of advice. . . . The differences in the use of 'good' in advice and choice are due to the fact that the problem to be solved is now someone else's. The adviser is not making up his mind what to do, but helping someone else to make up his mind. And this difference brings with it another. The relevant pro-attitude is that of the audience. But in other respects, the contextual implications are the same. To tell someone that something is the best thing for him to do is to advise him to do it, but

[11] *Ibid.*, p. 160.

not irresponsibly. The speaker implies that he has good reasons for his advice, that he knows what the problem is and that his advice is relevant. The same predictive and causal elements are present as in the case of A sentences; and advice may as before be given disingenuously, improperly, mistakenly or unfortunately, if one or other of the contextual implications is absent".[12]

And speaking of verdicts and appraisals, Nowell-Smith observes that appraisals are judgments which are made by reference to standards or criteria, but that they are judgments which do not mention the criteria, but imply conformity with them.

Thus examples of 'good' and of 'best' considered by Nowell-Smith are more usually non-moral uses such as in 'that would be the best course to adopt', 'that is the best dog', or 'best car', or 'best wine' and so on. Except for the first—which could equally well be or not be—these are not moral uses of 'best'. The sentence 'That would be the best thing to do' only expresses a moral statement when 'best' is short for 'morally best'; and similarly with 'good' and 'ought'. Nowell-Smith's accounts of the uses of 'best', 'good' and 'ought', in the contexts of 'best car', 'good wine', and prudential 'oughts', have some plausibility. Very often reasons are contextually suggested of the kinds and in the ways he suggests—i.e., relating to our wants or pro- attitudes, or to conventionally accepted standards, or to criteria with some sort of historical if not logical link with pro-attitudes. But the fact that this is true of non-moral uses of these words must raise a doubt whether it is also true of the moral uses of expressions containing 'good' and 'best'. 'Best thing to do' could refer to the best route, the most convenient mode of travel, the cheapest mode of travel, or that which best allowed the person to fulfil obligations to visit people. When, then, does the expression, 'the best thing to do', become a moral expression? Nowell-Smith attends much less to this question than one has reason to expect in view of its importance. However, he does discuss issues relevant to it well on in the book, when he sets out the discriminating characteristics of a moral, by contrast with a non-moral, pro- attitude.

How then are the moral uses of 'good', 'ought', 'the best', etc., to be discriminated? This comes to be restated as: How do moral pro- attitudes differ from non-moral pro- attitudes? Here Nowell-Smith seems to reply by asserting that any pro-attitude may become a moral pro- attitude provided it is held in a certain way. (a) Moral principles are dominant principles in the sense of being fairly consistently held and applied, and relating to important matters and not being over-ridden by principles other than moral

[12] *Ibid.*, p. 162.

principles. (b) This is qualified by the acknowledgment that a man's moral principles, whilst not always those according to which he acts, are such that he acknowledges them in his more reflective moods and feels remorse when he breaks them. (c) Moral principles are universal in the sense of being applied without exceptions, except where other moral principles are involved. (d) They are relatively long-range principles or policies.

This account won't do for a good many reasons. It is vague. It is circular—a moral principle is explained as one which is legitimately over-ridden only by another moral principle. And in so far as it is clear enough to be tested, it excludes and includes too much. The moral standpoint and the standpoint of egoism are distinct; yet on Nowell-Smith's criteria for a moral principle, egoism—a rational, relentless egoism—could qualify as a moral code. On the other hand, moral principles not applied with strict universality would be denied the title moral, in quite an arbitrary way, by such criteria. It is only higher level moralities, as Ginsberg points out in *The Diversity of Morals,* that display the universality modern writers such as Hare and Nowell-Smith attribute to morality as such. But these points need not be pressed, as they have been developed at length elsewhere. However, it is to be noted that Nowell-Smith's attempt to discriminate the moral from the non-moral involves him in some very queer views about the nature of immorality and of moral weakness. It leads to the conclusion, for instance, that some men will have no moral principles, and hence presumably cannot be immoral—and these individuals will be more than simply the handful who on any showing might fairly be said to be amoral.

This means that Nowell-Smith's account is, as it stands, inadequate as an account of the moral uses of 'good' and 'ought'. If we consider how it fares as a basis for accounting for the rationality of morality—of reasons in ethics, and of disagreements, particularly those relating to ultimates, we find that it is no more satisfactory.

Nowell-Smith's account of reasons in ethics has something of the same detachment from reality as has his discussion of 'good' from the moral uses of that expression. The most characteristic situation in which we seek reasons for and against moral judgments is in the personal situation of moral perplexity and deliberation preceding a judgment, decision or choice. Nowell-Smith's account, even in the matter of choice, has something of the air of a report of an outsider looking in—with a stress on *explaining* reasons for a choice rather than *justifying* the choice—and as a result the moral situation does not seem to be fully seen and portrayed. To say 'that is the best course' is, on his account, to imply that one has reasons, but it is not to state them. But what

sort of reasons are implied and what makes them adequate reasons?

We may see the sorts of reasons Nowell-Smith has in mind by considering a G sentence—e.g., Clergymen are trustworthy people—and the sorts of reasons which might be advanced in its support; and again by considering a straightforward non-moral appraisal use of 'good'—e.g., That is a good car. In the latter case the claim would be supported by reference to 'natural' attributes such as its efficiency, economy, comfort and perhaps elegance, etc. These would be factual considerations—or partly factual, partly themselves evaluative in a less controversial area—relating to pro- attitudes, or to standards or criteria which are related to pro- attitudes. One could challenge the judgment by questioning the facts—it isn't a reliable, or an economical car—or by challenging the standards used as a basis of the judgment—e.g., if one were a millionaire, economy might be rejected as irrelevant and greater stress placed on comfort, reliability and style. And this would usually be a dispute that would be settled by reference to the pro- and con- attitudes of the disputants, or by reference to a factual consideration of what is commonly counted as a relevant standard in judging the goodness of cars. But if the dispute turns on what ought to count as relevant criteria, then the pro- and con- attitude account is less satisfactory. Efficiency in performing its function would suggest criteria; but these at most would be minimal criteria; and there may well be disputes about what is or ought to be the function of the car. Aesthetic and moral value judgments—about the importance of beauty and of safety—are liable to intrude, and the dispute become one of a significantly different character, one about ultimate values rather than one about pro- attitudes. This, of course, still has to be shown; and I shall try to show that such disputes about ultimate values are not simply reducible to or explicable in terms of pro-attitudes by reference to an example of genuine moral perplexity. With this example relating to the car, I am content to bring out how the character of a dispute about 'good car' may significantly change if pressed back far enough; and change from one obviously related to pro- attitudes to one not obviously so related.

Now Nowell-Smith is concerned to contend, in brief, that to assert that a certain thing or course of conduct is good or obligatory is to express approval in an impersonal way implying reasons for the approval. In the case of choice, the reasons he suggests relate to one's own pro- attitudes; in the case of advice, to the pro-attitudes of the hearer. With the moral uses of these expressions, the pro- attitudes ultimately referred to are moral pro- attitudes as explained above. They are said to be of two general kinds—principles adopted to harmonize our pro- attitudes with one an-

other, and those designed to harmonize them with those of other people. These are thought of as being based on more ultimate moral pro- attitudes, many of which are claimed to be natural. The reasons in explanation—and presumably in justification—of moral judgments are represented as being these pro- attitudes and of course factual considerations relevant thereto.

This is unsatisfactory both as an account of reasons and as an account of the nature of disagreement in morals. To explain the first point—its unsatisfactoriness as an account of moral reasons—we need only to consider an example of moral perplexity and the sort of deliberation and doubts to which it might and often does give rise.

Consider the problem faced by a poor, Roman Catholic woman, already the mother of a large family, who, because she has been told that another pregnancy will almost certainly kill her, is considering having herself sterilized, unknown to her husband. She knows that her husband, a devout Roman Catholic, would never agree to either sterilization or less drastic forms of birth-control, and that he will nonetheless insist on maintaining full marital relations, even at the risk of her life. She herself is a devout Roman Catholic. On the one hand she has to weigh the vast deception involved in choosing sterilization, the breach of the natural law in what she believes to be an abuse of the sex function, the sin against God—if such in addition is also involved—against the evils of not choosing sterilization, the evils which would befall her children if she were to die, and the fact that she would lose her own life. This would be further complicated by the certainty of the evils in the event of her choosing sterilization as opposed simply to a high probability of the evils resulting from her not being sterilized. How is she to resolve her perplexity? How is she to determine whether there are other relevant reasons, and which of the apparent reasons for one course or other is a real reason, and a more telling reason than some other?

If the woman is simply represented as pondering about the relative weight of the reasons noted here, and not about whether they are genuine reasons, then Nowell-Smith could, at this level and from an outsider's viewpoint, take note of the relevance of the various considerations noted; but how, in terms of his account, can the perplexed person begin to resolve such a perplexity? Nowell-Smith seems ultimately to suggest that the person has to ask herself: What is it I really want most? But this won't do. Obviously, in any straightforward sense of 'wants', the woman probably would most want to live; in an equally important sense, she probably wants to live, to care for her children and to remain in good standing with God. These in a loving parent and devout Roman Catholic would probably be equally strong ultimate pro-

attitudes. She might decide, as so many reluctant martyrs have done, that she most wants to live, even at the sacrifice of her good standing with God. After all, not all who decline the distinction of martyrdom are humbugs. But all this is not to say that she believes the course of self-preservation is the morally best course; morally she may most want—morally want—to obey divine and natural law. Thus, unless 'What she wants most' is construed as meaning 'what is morally best', it is false to claim that the resolution of the moral problem is to be achieved by discovering what one wants most. And if 'What she wants most' means 'What is morally best' then this is merely another way of restating the problem. Allusion to the alleged universality of moral pro-attitudes will not help here because in the martyrdom situation the martyr may think it to be his duty, but not the duty of every man in a like situation. Whether he thinks this or not may well turn on the seriousness of the evil which is to be avoided only by death. An objectivist such as Ross can do justice to the explanation and justification of the function of those reasons relevant in such a situation; so too can a believer in natural law; Nowell-Smith's theory, on the other hand, seems to give a distorted picture here.

Very often, of course, when we are faced by such fundamental conflicts, we begin to question one or other of the conflicting values. In our example, the woman may simply say, as so many Roman Catholics do, that the Church is mistaken about the natural law and unrealistic about birth-control, that its unrealism results from the Church's being run by celibate men with no real understanding of the relevant facts of life. In such a way much of the acuteness of the conflict could be mitigated. And Nowell-Smith's theory would be as able as any theory to accommodate this sort of resolution. On the other hand, the wrongness of deceit—or of certain varieties of deceit—might be questioned, or the value of life and happiness re-thought, and so on. It is here, where ultimates—moral ultimates—come to be involved, that Nowell-Smith's account becomes most transparently inadequate.

Nowell-Smith's account reduces moral deliberation to a queer sort of self-investigation, to the discovery of our real pro- attitudes or how we may best harmonise them. He argues, for instance:

> "Unless men had pro- attitudes, there could not be even rudimentary analogues of what we know as appraising, judging, or passing a verdict. For these involve the use of standards; and without pro- attitudes we should neither have any use for standards nor even be able to understand what a 'standard' was".[18]

But, as we have seen, moral perplexity is not perplexity about

[18] *Ibid*, p. 181.

what we really want. To ask: Is this what I ought to do? is not to ask the same thing, nor even the same sort of thing as: Is this what I really want? It is true that Nowell-Smith specifically qualifies his use of 'want' in this context, explaining that it is not a completely apt word; but as has already been argued, it is a false assertion unless 'want' simply means 'want as morally best', and then the claim is uninformative and takes us no further.

Again, a moral decision is a decision, and not merely a discovery, although it contains an important element of discovery. This may be seen by considering issues which raise genuine moral perplexity—as in the example already outlined, and as in the following. Consider deliberation about the morality of nuclear warfare. Is such deliberation simply the sorting out of our pro- and con- attitudes? Or, when a person deliberates about a more ultimate matter—whether to give his life over to deliberate, carefully planned self-seeking, or to organize it on the basis of doing all he can for the good of his fellow men—we should not think of his perplexity as being one relating simply to sorting out his pro- attitudes. If this were all he were doing, we should think that he was not perplexed about the specifically moral question.

Yet Nowell-Smith in many places shows that this is how he construes moral perplexity and personal deliberation; certainly this is how the logical analysis he offers suggests that moral perplexity should be interpreted. His introductory illustration is of a mother who is too seriously ill safely to suckle her child. In an unusual, refreshingly patriarchal light, he explains the husband's problem as consisting in discovering which he wants most—his wife's well-being, or the various goods which would come to the child at a great risk to his wife as a result of her continuing to feed the child. The patriarchal formulation of this problem is no accident. If moral deliberation is discovery of one's pro- attitudes, then this is how the problem must be formulated.

This view is again expressed at the end of the book in the passage quoted earlier in this paper, in which Nowell-Smith claims that 'the most a moral philosopher can do is to paint a picture of the various types of life' so as thereby to help you to decide what you really want to do with your life.

It is true, as already noted, that Nowell-Smith qualifies his talk about 'want' when discussing pro- attitudes and how they may best be generally characterised. And it is true that he does not equate moral pro- attitudes crudely with wants; but when everything is taken into account, his talk about deliberation involving moral ultimates does amount to talk about what we really want in something at least akin to the ordinary sense of that expression—it must, for otherwise his own account would come to be exposed to the criticism he urges against the objectivists, of

leaving a gap between the judgment that a certain course is the best course and our adoption of it. Alternatively, we must conclude that all his cumbersome terminology has taken him no distance, and that perplexity about ultimates—moral ultimates—is to be explained as perplexity about what is best, where 'best' is left uncharacterised; but this is an unlikely conclusion, and the former alternative is the one we seem forced to accept.

In outlining this criticism I have been concerned to press a difficulty about one aspect of Nowell-Smith's account of moral perplexity involving ultimates. Other difficulties concerning his account of moral ultimates arise in other ways, and are of equal importance. The unsatisfactoriness of Nowell-Smith's account on this score is again apparent in the context of his account of moral advice.

'Good' in the context of advice, we are told, is geared to the recipient's pro- attitudes. But moral advice is geared to the recipient's pro- attitudes only in the very general sense that it is moral and that most people have a pro- attitude towards being moral and towards choosing what is morally good—but only in this sense, and even here advice is not always geared to the recipient's pro- attitudes in that some people are contra- suggestible, and others have a con- attitude towards the moral (the latter seems implicitly to be viewed as impossible by Nowell-Smith); and in these latter cases our advice would not be necessarily affected by these facts about the recipient. In any case, if the claim that advice is geared to the pro- attitudes of the recipient amounted only to the claim that people have a pro- attitude towards the moral, then it would be a trivial claim. Since Nowell-Smith thinks he is saying something important, and since he intends his remarks to relate to advice involving 'good' in non-moral contexts as well as in moral contexts, he clearly must mean more than this; yet if he does, his assertion is patently false in respect of moral advice, although on the whole apt in respect of non-moral advice.

The point about moral advice, by contrast with much other advice, is that it is by no means geared to the attitudes of the recipient—not even in the limited sense specifically mentioned by Nowell-Smith of relating to and resting on an understanding of the recipient's problem—at least not as he necessarily understands it. (This is one of the things which make a general consideration of the uses of 'good' and 'best' in the context of advice so misleading in a discussion intended to throw light on the moral uses of 'good'). We may know that a person is determined to act wrongly; but that would not in itself cause us to alter our moral advice—unless the issues were very important. And we may know that a person has an unfavourable attitude towards charity and declare

that it is his duty to donate to a specific cause or to a particular person. And more generally, when we are asked for moral advice by another person, we do not ask ourselves: What are his pro-attitudes? What does he want most? — nor even — What would most satisfy his moral pro- attitudes? Nor, What would satisfy the conventional moral principles? Rather we consider what he ought morally to want. Similarly we consider, not what he considers his problem to be—although this would no doubt be a common starting point for offering moral advice—but rather what his problem ought to be. The latter is true even with much non-moral advice.

To consider, now, an example of how our approach to moral advice differs significantly to our approach to much non-moral advice, and how it is that Nowell-Smith's account harmonises so much better with non-moral than with moral advice. If a friend asked us which person he should marry—glamorous, blonde, intellectual but vain Sylvia, or motherly, sympathetic, patient Prudence—we should then, if no specifically moral issues were involved, consider the question: Which of the two states of affairs does Charlie really want? Which, taking everything into account, would give him most satisfaction? This is in sharp contrast with the moral problem and the moral advice which would be offered if, say, Charlie had promised to marry Prudence, had seduced Sylvia, who was now expecting his child, and loved and wished to marry neither. In this sort of situation, there would be a genuine moral problem calling for genuine moral advice; and in determining what is the best advice, Charlie's pro- attitudes, and even his moral principles, would simply be among the relevant facts, the latter, of course, being more important than many of the relevant facts, but not conclusively so. In offering the advice finally deemed to be best, the tactful adviser would try to relate it by persuasive methods to Charlie's pro- attitudes; but if the adviser were Sylvia's fiancé, or her father, or her priest, it might well be put bluntly in the form of demands with little embellishment of any kind. Yet Nowell-Smith is right in supposing that his sort of account of 'good' does involve relating advice and the recipient's pro- attitudes rather more closely than this consideration of the moral situation would suggest to be in fact the case.

Thus Nowell-Smith's account of moral advice must be rejected as unsatisfactory; and it seems to be unsatisfactory because it is the account which follows from the overall analysis of moral terms in terms of pro- and con- attitudes, thereby assimilating moral advice misleadingly with a certain type of non-moral advice. The person seeking advice wants to know what course to choose; at the same time, he wants a justification of the choice. In morals this cannot simply be given in terms of either the adviser's or his

own pro- attitudes, whether it be directly or in terms of some tenuous connexion. It cannot even be given in terms of their moral pro- attitudes, not even where these moral pro- attitudes are left undefined and seen as *sui generis*.

This latter point brings out the fundamentally erroneous character of Nowell-Smith's enterprise. A thing is not made moral by our having a certain attitude towards it; rather, our moral pro-attitude is something which ought to be a response; and it is a response which can be an inappropriate one as well as an appropriate one. Something is not made moral by our attitudes, whatever they be, although it is true that our manner of regarding something—as objectively moral or not—shows that we think of it as being in the sphere of morality. But this is distinct from whether it is or is not in that sphere.

However, it will no doubt be objected that Nowell-Smith is not claiming that our attitudes make something moral, but rather that for something to be moral is for it to be related in a special way to attitudes held in certain ways. It is true that this is the form of Nowell-Smith's assertion, but its impact is such as to make the above criticisms applicable and relevant. A consideration of moral advice—how it is arrived at and how it is offered and justified—brings out that it is not primarily geared to pro-attitudes in the ways suggested by Nowell-Smith.

As these criticisms of Nowell-Smith's account of 'good' in the contexts of choice and advice suggest, the most significant way in which his account is inadequate is in respect of ultimates—in particular, in respect of the justification of ultimates. This inadequacy may now most usefully be considered further.

Nowell-Smith argues that choices are decisions which are to be explained in terms of general rules or principles, which are general pro- attitudes, and that these general pro- attitudes may be reduced to a number of pro- attitudes, many of which are natural, these moral ultimate and more usually natural pro-attitudes including benevolence, self-preservation and private happiness. His manner of dealing with the problem of the justification of these ultimates is less clear At times he appears to seek to dismiss the problem as one which doesn't get going, because the question raising it is necessarily 'logically odd'; at other times he seems to go further and contend that it is logically impossible to question all ultimates.

To consider the approach to the problem in terms of logical oddness. Here he seems to contend that questions such as: But is benevolence really good? are logically odd in the way in which it is logically odd to ask of a person who has just said that the strawberries he is eating are nice whether he is enjoying them. His point is supplemented by the contention that pro- attitudes

such as that of benevolence are natural; although it is not clear just how far he wishes to tie the claim concerning the logical oddness of the question to the alleged naturalness of certain ultimate pro-attitudes. For my own part, I cannot see that the request for the justification of benevolence, or of any other ultimate moral principle, appears in any way logically odd, unless it is at the same time argued that the principle being queried is a natural pro-attitude. Consider the difference between the two questions: (a) I know that it is natural to approve of the elimination of suffering; and I do approve of it myself; but am I right in so approving? (b) I know that it is natural for people to feel goodwill for their friends and associates, and to discriminate against strangers, and I approve of this; but is it right to approve of such a limited mode of distributing goods, or should goods such as happiness be distributed over the greatest possible number? The naturalness of the pro- attitude seems to be necessary for the claim that the request for a justification is a logically odd question to have even initial plausibility. But even then, the argument from logical oddness will not do, and for the following reasons: (i) It is not clear that benevolence is a natural pro- attitude, or that all moral principles, including those of justice, can be reduced to benevolence and private happiness—and a demonstration of the latter would be essential if this move were to be satisfactory. (ii) It is also not clear just how the alleged naturalness of the pro- attitude rules out the question of the justification of the pro-attitude as being logically odd To take questions such as: Is benevolence good or obligatory? Is taking innocent life for gain wrong? Is egoism a morally good way of life? etc It is difficult to see how the fact that an attitude is natural makes such questions 'logically odd' in any significant sense of that term. (iii) The concept of logical oddness, as we have seen, is an indefensible concept, being little more than a high-sounding name for referring to a confusion of things, ranging from logically meaningless questions to unnecessary and even unusual questions. And, it should be noted, if the concept of logical oddness does not perform a function in this context, then it plays no important rôle in Nowell-Smith's theory.

Thus an attempt to deal with the problem of the justification of ultimate moral principles by legislating the question out of order as logically odd is therefore unsatisfactory, even where it is associated with the claim that the ultimate pro- attitudes are natural. However, what is perhaps his main argument is a different one, and one which amounts to a re-application of the fashionable arguments against the problem of induction in the sphere of moral ultimates. Here Nowell-Smith argues that it is logically impossible to question all ultimates at any one time, for the demand for a justification is only meaningful on the basis of

an assumed standard. Thus to ask for a justification of benevolence is to ask for it to be related to pro- attitudes not then being questioned. Nowell-Smith associates this argument with a belief that certain ultimate pro- attitudes, including benevolence, will not in fact be questioned, because they are natural; but this belief is incidental to his argument.

There are two objections which are to be urged against such a rejection of the problem of the justification of ultimates. Firstly, it is a rejection which rests upon a false view of the nature of the demand for a justification of moral ultimates. With ultimates in other spheres—e.g., criteria in determining good wines—it is true that a request for the justification of the standards themselves is a request to relate them to pro- attitudes or to goods of another kind. That is to say, in these non-moral cases, the request for a justification of standards is a request for an explanation of what they are good for, or for the purpose they serve. But the demand for a justification of moral ultimates is not at all like this. The question—Is the elimination of unnecessary suffering really a good?—is not a request, expressed in an incredibly misleading way, for a demonstration of what the elimination of suffering is good for, or for an indication of its purpose, or of the pro- attitudes with which it will accord. That is to say, the request for a justification of ultimate moral standards, unlike the request for the justification of most other standards, is a request involving the categorical and not the hypothetical use of 'good'. It is simply to misunderstand the nature of the demand for a justification of moral ultimates, to assimilate it to requests for justifications of other 'ultimates'; and whilst it is no doubt true that it is difficult to explain in other language what a demand for a justification of moral ultimates is a demand for, it will not do simply to evade the problem by misleadingly assimilating it to problems from which it is vastly different.

This in itself would seem to be an adequate answer to Nowell-Smith's attempt to dissolve the problem of the justification of moral ultimates. However, another lesser consideration which bears against his contention might usefully be considered. Nowell-Smith allows that it is logically possible to question all apparent ultimates, but not all at any one time. And this in itself is to concede enough for the problem of the justification of ultimates to get going as a real problem needing a solution and not a dissolution. The morally perplexed person is very often a person who does doubt all ultimates, and, by doing so, whether there are any such ultimates. He may one day doubt the principle of utility on the basis of an intuitionist's view of justice; and on another day, when faced by a different moral situation, he may question the intuitionist's view of justice from a utilitarian standpoint. And

in another situation he might well question both from the standpoint of some sort of natural law position. Different situations give these different positions rather different force, and uncertain persons could and do very easily change their standpoints in this sort of way. (The utilitarian, for instance, is very often the harshest man in condemning the 'unnatural', 'perverted' behaviour of the homosexual.) Now, once the person reflects on his uncertainty and doubts, he doubts all three or more positions; he may doubt whether there are ultimates at all—i.e., whether there are principles which are a source of justifications but not themselves in need of justification. And when he puzzles over this, he is not wondering whether there are principles which have their justification in their accordance with certain pro- attitudes or the like. Nor is he asking to be helped in some arbitrary way to settle on some one set rather than on some other set of principles as ultimates. He wants to discover what are the real ultimates. And this is at least a meaningful, and common want, and one to which Nowell-Smith, in terms of his account, is unable to provide an answer or explanation.

This means that Nowell-Smith is forced to contend that in the most characteristic moral perplexities and disagreements, not merely no reasons can be offered one way or the other, after the implications of the two principles or ways of life have been grasped, but one cannot be said to be more right than the other. As detached observers we are told that this is right which is thought to be right by the moral agent who understands all the facts—about his pro- attitudes, rules, and the like.

This means that, in the most important area of ethical theory and of moral life, Nowell-Smith has made no real progress on the accounts offered by Hare and Stevenson. He provides no real grounds for claiming that subjectivism can reveal morality to be essentially rational. And this is implicity admitted when he allows that 'moral disagreements' are a special species of disagreement.[14] Rather than explaining morality as being essentially rational and as having authority to oblige us, he provides an account which goes a long way towards dissolving morality and taking away much of the point of having moral expressions. In so far as he relates moral principles to human nature, to that extent he gives them some point, although revealing them to be a very misleading way of expressing what could, on his analysis, be more clearly expressed in non-moral terms; but he takes away the point of being moral from the person who says—I am going to do what I really want to do; I refuse to be bound by moral considerations. After all, why should he, if Nowell-Smith's account of ethics is

[14] *Ibid.*, pp. 194-5.

right? Or alternatively, how can he but be moral? If Nowell-Smith sought to escape these criticisms by stressing the function of moral principles—that of harmonizing our own pro- attitudes with one another and with those of other people—he would not thereby save it from them, but would expose it to further criticism as a variety of ethical naturalism. A person may simply not want the sort of harmony of his pro- attitudes—with one another and with those of others—which comes with the adopting of certain pro- attitudes as moral principles. And to such an amoralist—and there are such people—Nowell-Smith in terms of his account, not merely cannot offer reasons for morality and the authority of morality, but also, as a logician of ethical language, he cannot even feel that they are mistaken in their stand.

This introduces the next serious shortcoming of Nowell-Smith's account which needs to be considered here, namely, that it fails to explain the authority of morality, and, *ipso facto*, of moral obligation as obligation.

Nowell-Smith criticises Hare's account on many grounds, among them that it fails to explain the authority of moral discourse. Yet, unless his own theory is essentially tied to a theory of natural pro- attitudes, he himself offers no more than does Hare in this regard; and even with a theory of human nature, he offers only an explanation of why moral principles are thought to have authority rather than why they do have it

To explain: It might be said that, in a sense, all pro- attitudes have authority—and lack it. Moral imperatives differ from the imperatives of other pro- attitudes in that they have an authority which these others lack. Why? If we look at Nowell-Smith's account of them as simply pro- attitudes held in a certain way—dominant, universal, etc.—this is puzzling, for he construes them merely as ordinary pro- attitudes held in a special way, and in a way which in no manner suggests that it confers added authority upon them. Why should they have additional authority when held in the manner Nowell-Smith characterises as the manner of moral pro- attitudes? This question is even more pertinent when it is seen that the manner of holding them is characterised as holding them *as if* they had this added authority, for this is part of what the criterion of dominance amounts to. In a sense Nowell-Smith's reply to this question seems to be that that is what or part of what is meant by calling a pro- attitude a moral pro-attitude; but obviously this is simply to restate the problem, not to answer it. It becomes again: Why should any pro- attitude be held in this way? Nowell-Smith seems not to have an answer here. His allusion to the reasons why we have moral rules—as

rules of personal success and as rules of social co-operation—is not peculiar to or characteristic of moral pro- attitudes; nor is it true of all moral pro- attitudes that they are one or other of these; nor is it true that social harmony needs the sorts of principles for its achievement that Nowell-Smith characterises as moral pro-attitudes. In any case, to allude to such rules is to give a non-moral justification of morality, already adequately enough refuted by Bradley[15] and Prichard[16]; and yet to give a moral justification for having morality is simply to refer the problem back a further stage.

Thus Nowell-Smith's account of the logic of moral language fails to explain, or to allow to be explained, the peculiar authority of morality. He fails to explain why we have moral standards, and why those who have no moral principles should be induced to adopt some, and particular ones at that.

Another way of expressing certain of these points is to argue that Nowell-Smith fails to explain and account for moral obligation. If moral obligation is merely conformity with a pro- attitude which we have adopted, why should we conform? Why should we be obliged by it? Nowell-Smith's account of the use of the word 'ought' seems not to help here. And notice that his account leads to another paradox, namely, that moral principles are things we can create and adopt. In fact, they are things we feel forced upon us by outside facts. Our conformity or non-conformity with them is something about which we can exercise choice; but we cannot exercise it about them. If Nowell-Smith's account were correct, we should feel very differently about accepting our moral standards. We simply do not set out to make or choose them; rather we set out to discover the true standards, pleasant or not; and discovery, not adoption, is the primary concept here.

Nowell-Smith's manner of dealing with the problem of moral ultimates exposes him to many additional criticisms, of which one of the more significant may usefully be noted here. If Nowell-Smith's account were to be accepted, it would imply that moral judgments, in particular the moral censure of others, is highly presumptuous and involves arrogance, and arrogance of a kind of which few people would be guilty if they realized just what they were asserting when using moral language. There is something seriously defective in an account which suggests that moral language causes us to do what we should rarely feel entitled to do, if we were to understand what it is we are doing by means of it. Nowell-Smith's account implies that moral censure consists in our attempting to impose our pro- attitudes—adopted and held in a

[15] *Ethical Studies*, ch. 2.
[16] *Moral Obligation*, ch 1.

certain way, it is true, and possibly natural and shared by others—but he nonetheless implies that we are trying to impose our pro-attitudes when we censure others morally; and clearly we should not feel entitled to do this in respect of those of our pro- attitudes not adopted as moral. Rather, we should feel it to be presumptuous to censure someone for not conforming with our partiality for lemons, even if there were a natural pro- attitude towards eating lemons. Why then should it be permissible to seek to impose our moral pro- attitudes, but presumptuous and even arrogant to attempt by censure to impose non-moral pro- attitudes? The role of convention can scarcely be invoked here, for a man is still not arrogant if he seeks to censure another person on the basis of a moral code the rest of the community rejects. It is not presumptuous because of the nature of the claim that is being made—the claim is not a personal one, but one about the facts. If a man is out of step with the community, he is wise to reflect on his beliefs; but having done so, he cannot be claimed to be presumptuous in pressing his well-considered moral beliefs against the conventional code by censuring the conduct of those who act in accordance with the conventional code. The anti-vivisectionists are not arrogant, whether they be mistaken or not. Nowell-Smith fails to explain the propriety of such censure in terms of his account of the function of moral words. And notice, again, how dangerous his approach to the logic of the moral uses of 'good' and 'ought' in terms of parallels with the non-moral uses of these terms, proves to be.

Our remaining criticism relates to the fallacious character of the argument underlying Nowell-Smith's whole theory. The fallacy of the argument underlying this account lies in the move from the desired to the desirable. Nowell-Smith condemns—and with excellent reasons—the reduction of all pro- attitudes to the language of desire; but his arguments here do not invalidate the point of this criticism. What is naturally liked, or approved, or pro-attitudinized, is not necessarily best—not even where the pro-attitude is an indefinable moral pro- attitude. Further, as Nowell-Smith himself repeatedly allows, human nature could change such that there developed, for instance, a general pro- attitude toward the infliction of suffering on animals and young children. If this occurred, this would not make the infliction of pain on animals good—even if it were to become a dominant pro- attitude, etc, and one necessary as such for harmonizing our own and other people's pro- attitudes—for there is a much more significant step from having a pro- attitude—whether it be in the individual or in mankind in general—to the value judgment about the worth of that towards which the pro- attitude is directed. Nowell-Smith seeks to explain this step in terms of the value judgment, 'This is

good' being an impersonal form of 'I approve', and 'I ought' being an impersonal form of 'I shall', the story, of course, being much more complicated than this, but in essence this.[17] But 'I approve', even in an impersonal form, i.e., reinforced or backed by reasons such as a reference to a general, natural pro- attitude, is not the same as 'This is good', and the equation of the two remains an instance of the fallacy committed by Mill in moving from the desired to the desirable. In Nowell-Smith's writing, it is simply obscured by a particularly clumsy, confusing battery of unnecessary technical terms.

To conclude: Whatever the merits of Nowell-Smith's many objections to ethical objectivism, it remains true that the alternative theory he himself offers is exposed to most of the traditional objections generally thought to be fatal to the claims of ethical subjectivism. If this is so, then in view of the many repeated attempts to resurrect this position this century, it might be more profitable to reconsider the merits of ethical objectivism with a view to seeing whether objectivism permits of re-statement so as to escape the more significant of the many objections Nowell-Smith and many others in recent years have urged against it.

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[17] See, for example: "We can account for the objective formula, as we did in the case of 'nice' by saying (a) that 'X is good' is not only used in the context of choice and (b) that when it is so used, it implies a great deal that is not implied by 'I approve of X' and is expressly denied by 'I happen to approve of X'. It implies that my approval is not an unusual one and that I could give reasons for it. It implies also—what is a matter of objective fact—that the object conforms to certain standards which are generally accepted" *Ethics*, p. 179.

DISCUSSION

MORE ON THE ARGUMENT OF THE PARADIGM CASE

By C. J. F. WILLIAMS

I wish to reply on behalf of the APC to certain criticisms made by Mr. Robert J. Richman in the issue of this journal for May, 1961 (pp. 75 sqq.).

Richman maintains (p. 76) that "there is no logical absurdity involved in supposing that we don't *learn* the meanings of any terms at all", and that "on this supposition some or all terms which we understood might have no *denotata*". In other words, it is logically possible that I should be born knowing what "red" means, although there never had been and never would be anything to which the term "red" could be correctly applied. The difficulty here is to see what sense could be given to the claim *to know* what the word "red" meant. All Wittgenstein's arguments against the possibility of giving an ostensive definition of "pain" would apply to this "knowledge". Suppose two people each claimed they knew what the word "red" meant, although in fact there were no red things, how could we begin to establish that they both thought it meant the same thing (had the same meaning), let alone that each or either of them was correct in thinking so? The normal way in which we determine whether a person knows what "red" means is by noticing whether or not he agrees for the most part with his fellows in what he calls red. If there were no true judgement which could be made of the form "x is red" (and to say that a term which we understand has no *denotata* amounts to just this), there could not be that agreement in judgements which Wittgenstein saw had to be presupposed for there to be agreement in concepts. (Of course, this argument assumes that the meaning of "red" cannot be given by verbal definition, or by indicating a gap in the spectrum—that it is in some sense "simple". There is no lack of criticism of the APC on grounds that "simplicity" in this context is unintelligible. Richman's argument, however, accepts the notion of simplicity, but affirms that the meaning of even genuinely simple terms could be known without being learned.)

The meaning of "pain" is learned, or knowledge of its meaning exhibited, by means of various sorts of continuity between natural pain behaviour and use of the word. Knowledge of what "red" means is also exhibited by various sorts of behaviour, but,

in so far as "red" is admitted to be a "simple" concept, this behaviour must be related in some way to red objects. The man who claims to know what "red" means would have to shew either that he can discriminate between red objects and others, or that he knows what types of objects are generally called red, or any of a number of criteria which need not be delimited *a priori*. That there is *some* sort of connection, mediate or immediate, between his use of the word and objects which people who are not colour-blind would recognize by sight as red, is, I think, a necessary condition of our saying that he means by "red" the same as we do.

Wittgenstein (P.I. § 312) imagines that "the surfaces of things around us (stones, plants, etc.) have patches and regions which produce pain in our skin when we touch them. . . . In this case we should speak of pain-patches on the leaf of a particular plant just as at present we speak of red patches". What Wittgenstein is envisaging is not the replacement of the behavioural criteria which now belong to the use of "pain" by other "objective" criteria of the type he is describing, but the addition of the latter to the former. If replacement had been what he imagined, he could not have allowed that 'pain" in our talk of pain-patches had the same meaning as our previous use of the word in expressions like "He is in pain". Now we could imagine a shift in the opposite direction in the case of the word "red". Every time we noticed anything red, or imagined ourselves doing so, it could happen that we had a feeling or sensation which found its natural expression in, amongst other things, a delighted gurgling. It might then be possible for us to use the expression "I am seeing red" in a similar way to that in which we now use the expression "I am in pain". Nevertheless, unless this use of the word "red" was an extension, not a replacement, of our present use of "red" to speak of red objects, it could not be allowed that the word still had the same meaning. If it were a complete replacement it might be possible—indeed it would be necessary—to claim knowledge of what "red" means without there being anything one could point to with the words "That is red". And perhaps it is a picture of something like this that Richman has in mind when he imagines a person who is led to understand the meaning of "red" by intravenous injection, or the like, without "red" having any *denotata* (p. 77). But if this imagined situation became a reality the word "red" would have acquired a totally new meaning, and the possibility—say rather necessity—of there being no "red" things in *this* sense would have no bearing on the existence of red things in the normal sense, with which the APC is concerned.

Another criticism made by Richman supposes that the APC is powerless against a man who says 'Nothing is *really* red".

"Granted a doctrine of ostensive definition [which doctrine?], granted that the meaning of some terms is taught by exhibiting examples, still all that is required to show the meaning of 'red' are cases of *apparent redness*" (p. 77). This is surely untrue. It ought by now to be generally admitted that non-committal assertions like "This *appears* to be a sparrow-hawk", or "This looks red" are parasitic on fully committed assertions like "This is a sparrow-hawk" or "This is red". If the ascription of redness involves reference to a paradigm case, *a fortiori* so does ascription of apparent redness. "This looks red" is an appropriate equivalent for saying, e.g., "If I were sure that the lighting conditions were normal I would say 'this is red' ", and to say this is to claim that I can detect really red objects when lighing conditions are normal. How could such a claim be shewn to be correct except by successful recognition of genuine instances?

Mr. Richman has other criticisms of the APC, some of which are valid, some not. I draw attention to the faults in the particular criticisms I have discussed because they relate more closely to the inner structure of the APC. Whether or not the APC can have useful, non-trivial applications, would introduce wider topics, which could not be dealt with in the same summary manner.

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COUNTERFACTUALS AND TRUTH

By R. J. PINKERTON

In "The Problem of Counterfactuals" (*AJP*, May, 1961),[1] Mr. R. S. Walters has set out an interpretation of the way in which "the truth of counterfactuals, properly interpreted, is an amalgamation of factual truth and necessary truth (or validity of argument)". I would like to question some of the details of this argument.

In Walters's opinion, those conditional sentences which express counterfactuals, but are not used truth-functionally, are elliptical, and require the specification of certain "factual assumptions or presuppositions" for the completion of their sense. Any such conditional, variously described as an "implicit inference" and a "truncated argument", is necessarily incomplete, and cannot be said to have a clear meaning until the assumptions or presuppositions (which form the premisses of the inference) are made explicit. It is suggested that the word "counterfactual" (used strictly) should

[1] Page references in brackets refer to this article.

refer, not to a conditional statement, but to a "complete argument" (p. 36).

On this view, then, a counterfactual is taken as *including* what would ordinarily be said to be the grounds of the counterfactual, and knowledge of these grounds is said to be essential to understanding the counterfactual, and even (though Walters does not bring this out explicitly) to knowing *what* counterfactual is in question. Apart from the difficulty of how we could possibly discover the grounds of something we do not understand, this theory makes it impossible to say—as we do want to say—that it is possible for people to agree on *any* matter (hence, on any "counterfactual" matter) without *necessarily* agreeing in their grounds or reasons. Walters's criteria for the identity of counterfactuals are such that it is impossible to know what the counterfactual is until we know the grounds, and any difference (other than a purely verbal one) in the grounds (unstated premisses) of two apparently identical counterfactual assertions means that the agreement is *merely* apparent. So we have the surprising conclusion that it is never possible to say the right thing (counterfactually) for the wrong reason.

Walters agrees that this result is counter-intuitive, but suggests that this is only because we lose sight of the fact that counterfactuals are really truncated arguments. But it is easy to construct examples where the point of using the counterfactual is precisely *not* to assert (incompletely) a particular argument. In deciding whether to allocate a further £50,000 to advertising, a Company Board might examine the adequacy of its advertising allocation for the previous period. Arguing on various distinct grounds (market research, experience of subsidiary companies, fortunes of other firms, industries, etc.), and each remaining unconvinced by some of the arguments, the Directors might nevertheless agree that the allocation had been inadequate. The Managing Director might at this point say, "Let us not waste any further time over these arguments, because it is at any rate clear that if we had spent £50,000 more on advertising last year profits would have been higher. We are agreed on this, and this is all we need to know at this stage to decide our policy."

To read the counterfactual here as the incomplete expression of a particular argument makes nonsense of the whole assertion. Just what argument has the Managing Director surreptitiously reasserted? Or, just what blunder has he committed in representing the agreement of the Board in this way? (That there must be *some* valid argument, if the statement is to be true, is not to the point, which is precisely the confusion between some (any) argument, and some particular implicit argument.)

Walters claims that the contention that two counterfactuals, of the forms "If A, then C" and "If A, then not C",[2] cannot both be true is a variant statement of the same theory of counterfactuals as the contention that the grounds of a counterfactual are not part of its meaning. He gives, however, no further reason for thinking this than the fact that they both make an immediate appeal to intuition. I should deny that "a demonstration that two conditionals of the kind given may be true together is at the same time a demonstration that we do not understand the meaning of given 'counterfactuals' without knowing the grounds on which they are asserted, allowing only that a demonstration of the latter would establish the former, for the reason that on the latter view the "meanings" of two conditionals of the kind given might well be independent of one another.

If the two theories were identical, the "demonstration" would seem to be easy, since there are plenty of cases where we should find it difficult to deny that two competing counterfactuals are both true. For example, why should we object to the conjunction of the following: "If there were round squares, they would be square", and "If there were round squares, they would be round"? Or, a more interesting example:[3] "If there were a perpetual motion machine, it would dissipate energy", and "If there were a perpetual motion machine, it would not dissipate energy".

We need to allow the truth of subjunctive conditionals such as these, if we are to avoid infecting *reductio ad absurdum* arguments with paradox. It would surely be undesirable if we were unable thus concisely to represent the two main branches in the strong *reductio ad absurdum* type of proof. In the examples given, we might be inclined to say that the necessity of the pairs of conditionals shows the impossibility (logical, and causal or empirical, respectively) of the suppositions. In those arguments, however, where the supposition yields a contradiction only when taken together with other propositions held to be merely contingently true, we should with justification be concerned with a proof of no more than the *falsity* of the antecedent.

While I agree, then, that there is no simple relation of contrariety between competing counterfactual conditionals, I nevertheless see no reason for allowing *any* true universal proposition to "sustain" a counterfactual, if by "sustain" we mean that the relevant counterfactual must be true. Consider Walters's logical analysis of a possible case (p. 37):

[2] For convenience, I shall call subjunctive conditionals or counterfactuals of these forms "competing", but do not mean to imply by this that they cannot play a draw.

[3] Suggested to me by Professor John Mackie.

"Suppose", he says, "that both

(P) All ravens are black, and

(Q) All surviving in snowy regions are white,

are true; and, further, that it is false that ravens survive in snowy regions. If X asserts that 'If ravens had survived in snowy regions, they would have been black', and assumes (P), then his counterfactual is true. If Y asserts that 'If ravens had survived in snowy regions, they would have been white', and assumes (Q), then his counterfactual is true as well."

The suggestion here that the assumption of (P) is sufficient for the truth of X's counterfactual and that the assumption of (Q) is sufficient for the truth of Y's counterfactual, is misleading, because the ascription of truth to the counterfactual implies that it is a statement, not an argument, and, accordingly, we might be led to think that the relation between (P) and X's assertion, and between (Q) and Y's assertion, is one of entailment. Since X's counterfactual is true because (P), together with the antecedent, entails the consequent (cf., p. 35), one might expect the counterfactual itself to be entailed by (P). (Cf., CCKpqrCpCqr.) But this would be to forget that the counterfactual is *really* an argument; and although no explicit example of a complete counterfactual is given, the sense in which (P) sustains X's counterfactual seems to be that (P) is itself part of the counterfactual (in this case, sole premiss) and entails whatever follows the "so" or "therefore". This remainder must be non-subjunctive, else the analysis would generate an infinite regress. Thus the complete counterfactual would have to be of the form "All ravens are black; so, if ravens survive in snowy regions, they are black."

I do not deny that this reduction of subjunctive conditionals is consistent, but simply insist that it does not show conclusively (as would the demonstration of an entailment relation) that if (P) is true, then X's assertion *must* be true. This conclusion would depend, not upon purely logical considerations, but upon our accepting the implied reduction as a correct analysis of X's assertion that if ravens had survived in snowy regions, they would have been black.

Walters seems to assume fallaciously that, because the ability to sustain counterfactuals will not serve as a criterion for distinguishing types of universal propositions, the distinction between law-like and accidental universals is irrelevant to determinations of their truth and falsity (cf. p. 40). But surely I could dispute the truth of X's counterfactual without denying the truth of his "grounds" (P) by pointing out:

(i) that his assertion involves an implicit extrapolation, since (P) and (Q) both taken to be true entail that it is false that ravens

survive in snowy regions (given certain trivial information about colour incompatibility which Walters assumes for his inconsistent triad on p. 38; although he here curiously adds the falsity of "ravens survive in snowy regions" as *further* information). So given (Q), (P) tells us no more than that all ravens in other than snowy regions are black; and

(ii) that this extrapolation is unjustified, for reasons to be found in the science of zoology, where a difference in the relations of (P) and (Q) to other propositions of the science is established.

Walters's discussion of this point is vitiated by the assumption that his opponents must believe in the contrariety of "competing" counterfactuals, but his own conclusion, that given (A) as true, then (P)—rather than (Q)—is false, is rather uncomfortably like a concession that if X's counterfactual is to be counted as true, then Y's must be true in some stronger sense. But this distinction of different senses of "true" is explicitly and wisely eschewed.

If we determine the truth or falsity of our subjunctive conditionals according to the conditions set out by Walters, it is difficult to know how we should account for the apparently conclusive situation which sometimes arises when a supposition which we take to be counterfactual turns out not to be contrary to fact after all. We are told (p. 44) that the conditional "If this metal had been heated, it would have expanded", should normally be completed by the proposition "All metals expand when heated", and, "depending upon whether or not we regard this as true, we decide the counterfactual as true or false". Suppose that I believe that any metal does, under any conditions, expand when heated. On the basis of this belief, which is false, I assert the conditional mentioned. Walters accordingly marks my conditional statement as false. If it were now revealed that this metal had in fact been heated, and did expand, it should be possible for me to object that what I asserted had not been *false,* although it was certainly inappropriate, and should now have to be re-phrased so as to withdraw the implication that the metal had not been heated.

This raises a special problem for Walters, because he allows a sense of "counterfactual" in which the antecedent need not be false (p. 33). But the recommended analysis of "If X were to be Y, it would be Z" into an argument may show it to be "false" in a case where X is both Y and Z. This is possible because one may happen to assume a particular set (S) of contingent propositions which either contains some false propositions, or, supplemented with "X is Y", does not entail "X is Z".

Our determination of the truth or falsity of a counterfactual, according to this theory, is quite independent of any information that is not available at the time the counterfactual is first

asserted, unless the new information establishes that we were previously in error as to the truth or falsity of *the initial factual assumptions*. (New information can have no bearing on the validity or invalidity of the implicit argument.) Suppose, however, that someone had asserted in 1945 "If the A-bombs had not been dropped on Hiroshima and Nagasaki, the war could still have been brought quickly to an end". What logical objection can there be to regarding the judgment as strengthened by later information that the Japanese had made overtures for peace before the bombing; even though the judgment was originally based on inadequate or false information which is quite independent of the later knowledge? Am I precluded, by reason of seeking to support or improve on another's argument, from agreeing with him, just in case his assertion has a counterfactual or subjunctive form?

In this note I have not denied that subjunctive conditionals involve an implicit reference to argument, but I have sought to question Walters' account of this relationship. In particular I have argued that his theory involves an unacknowledged alteration in our employment of certain concepts (e.g., "same judgment", "truth and falsity"), for which no satisfactory reason has been given. I allow that we can say that knowing the meaning of a subjunctive conditional sentence involves knowing that there must be less sophisticated sentences in the language which can be put in certain characteristic relationships with the conditional. I suggest that this is not the same as saying that to know the meaning is to know what these sentences are. Nor is it the same as saying that the conditional refers to a particular set of these sentences, much less that a counterfactual actually includes (though tacitly) some such set.

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THE SUSTAINING OF COUNTERACTUALS*

By J. L. MACKIE

If it is a causal law that whenever potassium is exposed to air it bursts into flame, then we can say of a particular bit of potassium (which has not been exposed to air) that if it had been exposed to air it would have burst into flame. But if, as a simple matter of fact, all the coins in my pocket are shiny, we cannot say of some other coin that if it had been in my pocket it would have been shiny. That is, a causal law sustains counterfactual conditionals in a way in which a merely factual universal does not.

* A fuller treatment of this problem is contained in my "Counterfactuals and Causal Laws" to be published in *Analytical Philosophy*, ed. R. J. Butler (Oxford, 1962).

There is a widespread belief that if we recognize this difference we are bound to admit that a causal law is logically stronger than a simple universal, that it states a connection which is somehow necessary and not merely factual.

Mr. Walters, being critical of any attempt to set up different grades of universal, tries to show that, despite appearances to the contrary, all universals are equally able to sustain counterfactuals.[1] However, his discussion of this point seems unsatisfactory, and I shall show that the difficulty can be resolved in another way.

I assume, with Mr. Walters, that non-material conditionals, including counterfactuals, are to be understood as condensed and incomplete arguments: the counterfactual "If A, then C" is a condensation of the argument "Suppose that A; then (in view of certain true but unstated propositions) it follows that C".[2] Such an argument with a supposed premise may, of course, become formally valid when the unstated propositions are filled in, whether they are causal laws or not.

Thus in one case the completed argument might be: "All bits of potassium exposed to air burst into flame; suppose that this bit of potassium has been exposed to air; then it has burst into flame". In the other case it might be: "All coins in my pocket are shiny; suppose that this coin is in my pocket; then it is shiny". We are then inclined to say, with Mr. Walters, that in each case the (true) universal proposition, the supplying of which makes the argument valid, sustains the counterfactual.

But let us take it that in this second case, although it is true that all the coins in my pocket are shiny, I know this only by some process of complete enumeration, by having checked the coins actually in my pocket one by one. That being so, as soon as I add the (false) supposition that some *other* coin is in my pocket, I completely undermine the reason that I had for adhering to the universal, for this, being *another* coin, which is not actually in my pocket, has not been checked by this process of complete enumeration. Once I add the supposition that corresponds to the antecedent of a *counterfactual,* then, I have no reason for adhering to this universal, and therefore I cannot use this supposition and this universal as joint premises in an argument. Consequently I cannot assert the counterfactual conditional which is a condensation of an argument with these as joint premises. That is why the true proposition, "All the coins in my pocket are shiny",

[1] "The Problem of Counterfactuals", this *Journal,* May, 1961, especially pp. 39-42.

[2] I do not, however, agree that we cannot understand, use, discuss, or even decide such elliptical statements without completing them, and I should therefore not regard the unstated premises as part of the meaning of such a conditional; but this does not affect the present issue.

known merely by complete enumeration, does not sustain the counterfactual "If this (other) coin had been in my pocket it would have been shiny".

On the other hand, if we know that whenever potassium is exposed to air it bursts into flame, our reasons for believing this, whatever they may be in detail, are not undermined in any corresponding way by the supposition that this bit of potassium (which has not actually been exposed to air) has been exposed to air. The ultimately inductive evidence is related in exactly the same way to the universal proposition whether we add the (false) supposition or not. Hence this causal universal, supported in the way in which scientific propositions are supported, and this (false) supposition, can be used as joint premises in an argument, and consequently we can assert the counterfactual conditional which is a condensation of this argument. That is why the causal law, known in the usual inductive way, does support the counterfactual "If this bit of potassium had been exposed to air it would have burst into flame."

This means that whether a proposition does or does not sustain the appropriate counterfactuals depends upon the kind of evidence we have for it. Evidence of some kinds is undermined by the adding of a false supposition, evidence of other kinds is not. A proposition sustains an appropriate counterfactual if and only if the adding of the supposition (which corresponds to the antecedent of the counterfactual) does not do away with the reasons that we had for asserting the original proposition.[3] Someone might object that the problem has not been solved, but merely shifted: how can there be evidence which still supports the universal even when we add a contrary-to-fact instance of the subject term? But the evidence is related to this instance in just the same way as it is related to any instance that lies outside the evidence itself, and any inductive conclusion covers innumerable such instances. Thus, as I have said, the evidence is related to the universal in exactly the same way whether we add the supposition or not. If the objector asks what this relation can be, he is merely asking how there can be evidence which supports a universal that goes beyond that evidence, and this is neither more nor less than the general problem of induction. The problem of the sustaining of counterfactuals by causal laws is solved, therefore, in the sense that it is shown to be just the general problem of induction in disguise.

In particular, this account shows that we can recognize that

[3] Mr. Walters says (p. 39) that the difference between universals lies not "in any formal difference in the kinds of universal that they are, but rather in the relation which they have to other propositions in the body of science". I should accept this, adding at the end "or, in general, in the reasons that we have for asserting them".

causal laws sustain counterfactuals in a way in which "accidental" universals do not, without allowing that this sustaining of counterfactuals discloses any special *content,* over and above universality, in a causal law: there is no need to say that causal laws involve "necessary connections" or that they apply in some mysterious way beyond the realm of actual events.

University of Sydney.

PROFESSOR PRIOR ON THE AUTONOMY OF ETHICS

By J. M. SHORTER

In his article on the Autonomy of Ethics[1] Professor A. N. Prior finds himself "driven to admit that one can derive conclusions that are ethical in a quite serious sense from premises none of which have that character". I do not wish to deny this, but to draw attention to a certain futility about such inferences of which Prior does not seem to be aware. One must distinguish between the seriousness, ethically speaking, of the conclusion and the seriousness of the inference by which it is arrived at. Prior appears to think that this sort of inference is of some importance ethically because the conclusion can be of some importance. But this is not so. It may be the case that the conclusion is of use in deriving definite specific moral duties only if it is arrived at in some other way than by means of an inference from a purely non-ethical premise.

This is at least true of the sorts of cases he mentions, where the premise is not one of deontic logic. The point can be shown by taking two of his examples:

A *1* Tea drinking is common in England.
2 Therefore either tea drinking is common in England or all New Zealanders ought to be shot.

B *1* Undertakers are Church Officers.
2 Therefore undertakers ought to do whatever all Church Officers ought to do.

In A it is clear that a specific ethical duty can be derived from A 2 only if we know that the first half of the disjunction is in fact false. If it is false then we can derive the duty to shoot, for example, Professor Prior. If it is true, then A 2 is of no help to us in deciding whether we ought to shoot him or not. But if the

[1] This *Journal,* Dec., 1960.

first half of A 2 is false, then A 1 is false; and if A 1 is false then the inference A lends no support to the conclusion A 2. The making of the inference A is futile ethically either because it does not support its conclusion A 2 or because in so far as it does so it renders A 2 ethically useless.

The inference B is also futile, though in a rather different way. As Prior has pointed out, an undertaker can only derive a concrete duty for himself from B 2 if he also knows some proposition of the form "Church Officers ought to do ϕ". However, let us suppose that he simply knows:

3 Undertakers are Church Officers

and *4* All Church Officers ought to do ϕ.

He can then pass straight to "Undertakers (including myself) ought to do ϕ". That is to say, if he knows 3, he has no need of B 2. Only if he does not know 3, is B 2 of any use to him in deciding what his duties are. But if his only reason for believing B 2 is that he has inferred it from B 1, then he does know 3, which is the same proposition as B 1, and so has no need to make the inference. In other words, the step 5-6 in the following inference is otiose and would be better left out.

5 Undertakers are Church Officers

Therefore *6* Undertakers ought to do what all Church Officers do.

7 Church Officers ought to do ϕ

Therefore *8* Undertakers (including myself) ought to do ϕ.

Cases A and B are different in that in the first case, if the ethical conclusion is got by means of inference A, then no ethical duty at all can be derived, whereas in the second case the only ethical duties that can be derived are ones that could have been derived more simply without making inference B at all. It is true in both cases that the inference in question is quite useless for anyone who wants to decide what concrete action he ought to perform.

University of Canterbury.

REVIEWS

BERKELEY'S THEORY OF VISION. A Critical Examination of Bishop Berkeley's *Essay Towards a New Theory of Vision.* By D. M. Armstrong. Melbourne, Melbourne University Press, 1960. 106 p. 27/6.

In this work Dr. Armstrong attempts to prove three things: that Berkeley is mistaken in holding that distance is not immediately seen, that the objects of immediate visual perception are not, as argued in the *New Theory,* distinct from the objects of touch, and that the traditional commentators are wrong in seeing the doctrines of the *New Theory* as a logical half-way house to the doctrines of the *Principles.* There is a short final chapter in which Hume's arguments concerning the spatial location of sounds, smells and tastes are criticised. It is impossible to summarise the argument, as distinct from the theme, of the work, for Dr. Armstrong uses many arguments, believing that if one has the ammunition to deliver a volley it is foolhardy to rely upon a single shot. As a consequence we have a short work that is packed with arguments, quite a number of which will have to be answered by those phenomenalists who blithely tell us that we learn to correlate the deliverances of sight with the deliverances of touch, without saying what it is that we correlate and how we manage to do it.

The burden of these arguments is that, if we take Berkeley's contentions seriously, it would be impossible for us to have the picture of the world which we in fact possess. Thus, in Chapter 4, there are five arguments to show that if distance is not immediately seen we could not explain why it is that we *seem* to see things at a distance, could not estimate distance as accurately as we do, and could not easily correlate the deliverances of sight with those of touch. Then, after a digression on the immediate objects of touch, it is argued that, if visible and tactile extension were numerically distinct, we could not account for the commonsense conviction that the objects of the two senses are in fact spatially related.

The faults of the work are not in the arguments, but in what they take for granted. Dr. Armstrong seems to believe that by writing simply he is writing clearly, and does not seem to appreciate that strange things happen to simple expressions when metaphysicians use them. This comes out most clearly in his treatment of the key concept of "immediate perception". On page four, the notion of "immediately seeing" is elucidated with reference to that of the equally vague "visual illusion". I may seem to see a tomato, and I may seem to see red. Dr. Armstrong

says that if I am mistaken about the former a visual illusion need not have occurred, but if about the latter a visual illusion must have occurred. Hence we immediately perceive colours but not material objects. Following this clue, we should expect the question of whether or not we immediately see distance to be decided by considering whether or not, when we are mistaken about the distance of an object, we must say that visual illusion has occurred, but the arguments do not in fact appear to be directed at this target. Rather they seem concerned to show that the sense of sight is by itself sufficient to give us an idea of distance. Berkeley certainly denies this, but I am not sure that his denial rests solely upon the premise that distance is not immediately perceived by sight. It requires a further premise, that our senses will not of themselves make us go beyond the minimum claim we can possibly make. I suspect that Berkeley would have felt that the denial of this involved us in accepting the existence of innate ideas.

The notion of immediate perception has been used in so many way by so many philosophers, and the dangers associated with its use are now so well known, that Dr. Armstrong should have spent much more time telling us what he means by it, and what he thinks Berkeley means by it. This is doubly so when there are undoubtedly many who will be startled by the claim, made on page nine, that in discussing Berkeley and Hume on the objects of immediate perception, "It does not matter whether we take it that what are immediately seen are certain qualities of public objects, or, as Berkeley himself holds, are ideas or sense impressions". It does matter. We cannot appreciate Berkeley unless we realise that he was taking Locke's ideas seriously, and endeavouring to show how, if we *did* take them seriously, we would have to rethink our ideas of space. And we cannot make sense of Hume on sounds, tastes and smells unless we remember that he is concerned with showing us how we construct our knowledge of space out of ideas.

Furthermore, the dispute as to whether or not the objects of sight are distinct from the objects of touch becomes a queer dispute indeed if we look upon Berkeley as holding that they were distinct physical objects. It would suggest that Berkeley thought of the objects of sight as unfortunately distinct from those of touch, so that we just happened not to be able to touch the things we saw; but Berkeley did not, of course, think this. He would not admit that we could ever, in a strict sense, see and touch the same things. But in talking about this dispute Dr. Armstrong again takes too much for granted. He feels that it is quite clear what is being said by a man who does not deny that we can see and touch the same table in the colloquial sense, but who wants to say more than that the look of things is not the feel of things. He insists that

Berkeley's point is a point of substance, and dismisses Warnock's claim that Berkeley was really only emphasising the contingent connection between the deliverance of the various senses as reducing Berkeley to the level of "a linguistic Boy Scout". Berkeley would certainly have felt himself to be saying more than Warnock lets him, but it is not at all clear that there is anything left for him to say. W. D. JOSKE.

AN OUTLINE OF A SYSTEM OF UTILITARIAN ETHICS. By J. J. C. Smart. Published on behalf of the University of Adelaide at the Melbourne University Press, 1961. 51 p. 5/6d.

Professor Smart is concerned in this monograph to advocate the doctrine of Utilitarianism. He is not at all concerned to argue the truth of the doctrine as descriptive or explanatory of existing moral systems (pp. 40-41), but, like Mill most of the time, simply to urge its acceptance as a method of deciding what ought to be done.

He assumes 'the truth of some such meta-ethical analysis as that of Hare's *Language of Morals*' (p. 2), and suggests that the sorts of moral principle available to a philosopher who is (a) benevolent and who (b) does not accept, on philosophical grounds, a 'cognitivist meta-ethics' like Ross's, are much more restricted in scope than is often supposed (p. 2). That they are limited, in fact, to one of several slightly differing versions of the utilitarian principle. He holds, too, that philosophical argument aimed at the metaphysical foundations of a Natural Law morality is a legitimate device for persuading a benevolent Natural Lawyer to be a Utilitarian (p. 2).

The Utilitarianism he advocates is 'act Utilitarianism'; he holds, that is, that we ought to decide the rightness or wrongness of an action by its consequences, not by its conformity with a rule (pp. 4-5 and pp. 29-41).

It is very commonly argued (and I think rightly) that one of the central difficulties of Utilitarianism, whether interpreted in Smart's way or interpreted in the ways Smart is not concerned with, is the notion of 'happiness'. Smart's discussion of this topic is aimed at clarifying the notion sufficiently for the purposes of his general theme, and these sections ('Hedonistic and non-Hedonistic Utilitarianism', pp. 5-18, and part of 'Rightness and wrongness of actions', pp. 20-29) provide the closest argument of purely philosophical interest in the essay.

The argument here proceeds by an analysis of the difference between Bentham and Mill on the question of higher and lower pleasures. Smart argues that while the higher pleasures will in

general be preferable on utilitarian grounds, since they are more fecund than the lower, nevertheless an 'ultimate difference in attitude' (p. 15) is possible between Utilitarians on the question of whether the higher are *intrinsically* preferable to the lower. If some Utilitarian prefers poetry to pushpin, even though the quantity of enjoyable *sensation* is less in the former than in the latter, then there are, questions of fecundity aside, no arguments to be derived from the utilitarian system to make him change his mind. His is one of the possible versions of the doctrine.

There is at this point the following suggestion: '. . . for A to call B "happy", A must be contented at the prospect of B being in his present state of mind and at the prospect of A himself, should the opportunity arise, enjoying that state of mind' (p. 14). This suggests (since it is a remark on our usage) that we would be unwilling to say that people were happy whose behaviour we strongly disapproved of. But this seems to be a mistake. It is said that in the early triumphant days of the Wehrmacht its troops were extremely happy. But plenty of observers will admit that, but express the strongest disapproval of the Wehrmacht and all its works. Again, people commonly ask, in theodicy, why the wicked should be allowed to be happy.

But there seems to be a more important point, it might be argued, involved here. It runs clean counter to the nature of the utilitarian doctrine to suggest that what ought to be maximized is people's participation in activities the enjoyment of which the theorist approves of. For suppose people fail to enjoy what the theorist approves of, and in fact derive their happiness from behaviour which the theorist strongly *disapproves* of. Are they, on utilitarian grounds, to be coerced? That, presumably, on only one condition: namely, that there are grounds for supposing that the persons concerned will eventually come to enjoy the things in question, and *also* will admit that they are happier in their last state than they were in their first. Otherwise, of course, Utilitarianism would not be a doctrine advocating the satisfaction of people's wants and desires at all, but a doctrine advocating their participation in certain sorts of activities, irrespective of the way *they* felt about it. The activities in question would be decided by the taste of the advocate.

Smart makes some provision for an objection of this kind by arguing that there are 'minimal conditions' for happiness, namely that the person 'be fairly contented and moderately enjoying himself for most of the time' (p. 14), and this is an answer to the extreme form of the objection stated above. But it does not escape the objection that if 'happiness' means what Smart says it does or ought, in the system, to do, then the Utilitarian is not

concerned to advocate the maximization of people's enjoyment at all, but that of their participation in certain prescribed activities. Mill, too, moves uneasily here. Are the 'competent judges' of *Utilitarianism* (Everyman edition, pp. 9-10) the judges who prefer the higher pleasures to the lower, or are they judges of intelligence and sensibility as established by independent tests? If the former, then the claim 'Competent judges prefer the higher pleasures' is tautologous; if the latter, it is a shaky assertion of fact. One wonders whether, if the shakiness of the empirical claim were brought home to him, Mill would slip back into the tautology, and thus, indirectly, into the very un-utilitarian position which I am suggesting follows from this part of Smart's account of 'happiness'.

No detailed answer is given to the question, How do we determine what makes people happy?, and this, one feels, is a weakness in the exposition. Obviously, one can give *general* answers to this question, like, Enquire what are their strongest needs, and what their worst fears and pains (cf. pp. 21-22). In the world as it is there is, for the Utilitarian, enough to do in the way of providing food and education, and eliminating war and cancer.

But how is the question to be answered *in detail* in a world comfortably fed and housed and underworked and free from the worst pains of death? Perhaps the answer might be that by properly devised questionnaires each person's requirements, in their order of importance, could be determined. The information derived from this, appropriately processed and combined with information about the capacity of the economy, would yield the required information about who was to be supplied with, or released from, what.

But perhaps Smart would not accept this account at all. If not, we are still left unclear as to how we are to get adequate information about those 'total situations' upon which we are required to pass 'a purely ordinal not a quantitative, judgment' (p. 26).

On the other hand, if he does accept the method I mention, the problems of weighing 'the maximizing of happiness against equitable distribution' which he envisages (pp. 23-26) are considerably sharpened. For when it actually comes to the distribution of Holdens and TV's and afternoons at the football, there will be so many and complex variations in temperamental ability to do happily without and not to do happily without that, *quite apart* from utilitarian considerations about the length to which inequitable distribution can safely be carried, some decision will have to be made, *for practical purposes,* about the notion 'the maximization of happiness'. Are we consistently to favour those

more susceptible to joy and gloom against those less so? This is a decision to be made about the *meaning* of the utilitarian principle, not about how it is to be applied. And, as against Smart ('I must again stress . . . an approximate agreement is sufficient [for practical purposes]', p. 25) it seems to me that such a decision *would* prove to be a matter of practical importance.

Smart's prose is admirably clear, and his exposition lucid and systematic.

M. C. BRADLEY.

NEWMAN HISTORY AND PHILOSOPHY OF SCIENCE SERIES. London, Sheed and Ward, 1961.

This is a series of little paperbacks produced by the History and Philosophy of Science groups of the "Newman Association of Great Britain". The series aims to provide a general discussion of problems in this field and certainly does not limit itself to an exclusively Catholic point of view.

Other titles in the series released this year I mention after dealing with Harré's, which is the only philosophical one.

No. 9: THEORIES AND THINGS. By R. Harré. 114 p. 5s.

In this little book Harré is concerned to reject the positivist account of scientific theories. He characterises the problem as one of "prescriptive metaphysics", that is, as concerned "with the existence, presuppositions and priorities of conceptual schemes we might or should adopt" (p. 2). A person may take a position on such an issue "under a weight of rational discussion" but it is not possible to establish a solution by a conclusive argument.

The principle which he opposes to positivism he calls "ontological depth" and according to it "when we invent imaginary mechanisms to account for phenomena . . . we are entitled, under some conditions, to ask whether the imaginary mechanism or model exists" (p. 12). Suitable experiments will often settle such questions in the affirmative.

While indicating certain difficulties of a thorough going positivism that reduces all entities to constructs of sense data (p. 51-57), he is mainly concerned with the theory that admits existence to the objects of everyday knowledge but denies it to entities referred to in scientific theories which are not directly observable. The most forceful argument against this version of positivism is given on p. 32-33. If the terms in a theory which cannot be given "a cash value in terms of the results of observation and experiment" are not taken as referring to anything, then they can only serve a logical function and the propositions of the

theory would only express generalisations "whose function was solely that of linking up observables". Such a theory, Harré insists, could lead nowhere. 'The only new facts that could be discovered by its help must be facts of the same logical type as those already related together in those generalisations that make up its empirical side." The positivist analysis has only seemed plausible because there is quite a number of theories in which it may seem that no model has been used. However, the terms of such theories, on analysis, are seen to be metaphors which draw analogies with other phenomena: for example, "current", "energy", "force", "conductor", etc. It is these metaphors which confer on these theories their explanatory power and suggest laws covering new classes of events. If these theories, or those apparently describing unobservable mechanisms, are treated strictly as stating observed regularities and nothing more, then they would lack all explanatory power and would predict only the type of happening already observed.

It is unfortunate that so little space is devoted to this point and that it is not supported by a detailed consideration of two or three examples.

There are two methods cited by which we come to assert existence to the entities of an explanatory model. The most direct case is when we can follow through a "family continuity" (p. 58): that is, a series in which each member overlaps the next. For example, with the naked eye we see a drop of blood. With a magnifying glass there are still features visible which enable us to identify it as the same drop, but in addition new characteristics can be seen. So, as we increase the power of magnification through a series of microscopes, at each stage we can identify the object we saw in the last stage but see more as well. By carrying this series to the electron microscope we can draw a continuity from drops of blood to viruses, and if we unreservedly say that the one exists "it is equally legitimate to say that any object in optical family continuity with it exists" (p. 59).

When it is not possible to proceed by the above method we nevertheless sometimes assert the existence of entities that are not directly observable. If an explanatory model is particularly successful in accounting for already known phenomena and predicting new ones, we may come to believe that it exists. However, Harré does not seem to be entirely happy with entities of this kind, and he seems to considerably weaken the force of his thesis by making some unnecessary concessions.

In concluding that something exists by this second method we must bear in mind the general criteria for existence. Harré says there are two such criteria:

I. "A thing exists if it is spatio-temporally locatable" and II. "A thing of a certain kind exists if it fulfils our set of stable expectations for things of that kind" (p. 61 & 62). Criterion 2 is said to be the one to which we finally appeal. Criterion 1 is merely the necessary criterion for one kind of existence; for belonging to a particular ontological class.

He goes on to say that an electron only partially satisfies Criterion 1 and not Criterion 2 at all. 'We know roughly where they are . . . but we don't know even roughly what *one* will do, let alone what they are." The implication is that we can't say that electrons exist, although he does not explicitly say anything as strong as this. Now, in the first place, it is not true that we know nothing of how an electron behaves or what its properties are. In the second place, there is an ambiguity in the application of Criterion 2. What kind of thing are we claiming to exist when we say "electrons exist"? If it is taken that we mean that a very small material body exists, then if any properties assigned to electrons conflict with "our set of stable expectations" for material bodies, the application of Criterion 2 tells against their existence. But we are not obliged to think of electrons in these terms. If we are taken as meaning something which occupies a certain volume, has a certain mass and carries a negative charge of a certain magnitude, then, in saying that one or more electrons exist, we are referring to something that does conform to our stable expectation for *this* kind of thing. Certainly we don't know all the properties of electrons but neither do we know all the properties of viruses or genes and Harré is not worried about them. It is true that diffraction behaviour of electrons is puzzling, but the puzzle is about the kind of thing that they are, not about their existence.

The assertion that there are various "ontological classes" is somewhat puzzling. He says that mammals and magnetic fields belong to different fields of discourse and that this will influence the criteria for the existence of an individual of either kind. Now it is undoubtedly true that, when we have to go into the existence of something, that is when the thing is not directly observable in the context, what we consider depends on what sort of thing it is that we are dealing with. For example, we sometimes speak of the "existence of a tendency", and the truth of such an assertion would be entailed by suitable statistics. On the other hand, the "existence of photons" could not be entailed by any phenomena. If this is all that is meant by saying that things belong to different ontological classes, no objection could be raised, but this does not seem to be all that he means. His assertion that scientists need several ontologies (p. 66), that is, theories of existence, and his

reference to "various kinds of existence" (p. 62) imply that the word "exist" means different things in different contexts. This vitiates the point of his thesis, for the positivist can agree that there is *some* sense in talking about the existence of the entities of scientific theories as long as this is not the sense in which tables and chairs "exist".

To deny that "exist" changes its sense is not to deny that the criteria actually applied will differ from one kind of thing to another. In all cases where we do claim existence we have before us a description and we are saying that this description refers to something about which we can make true assertions.

A point at which Harré makes things unnecessarily complicated for himself is in discussing the logical character of the copula in "bridge-statements"; that is, statements which link observable facts with theoretical entities: for example, "pressure is average rate of change of momentum at phase boundaries" (p. 22). He argues that if the "is" be regarded as asserting identity, the positivist case follows. If these statements assert identities, then the theoretical term is a synonym for the description of the observable fact. That is, the theoretical assertion can be analysed into observable phenomena. However, this argument is fallacious. It is based on the assumption that all identities are identities of meaning, and this is false, as has been pointed out by Place and by Smart.[1] It is quite possible to have two descriptions which certainly don't mean the same but which, we discover, apply to the same individual. For instance, I can know the meaning of "the man next door" and of "the Prime Minister" and then discover that these phrases apply to the same person. In the same way, "average rate of change of momentum of molecules at the phase boundary" does not mean the same as "pressure of the gas", but this is not to say these phrases do not refer to the same thing.

By characterising such statements "synthetic identities" Harré could have avoided some difficulties. For instance, if "a gas is a collection of molecules in motion" is not an identity, does it follow that within a given jar there are two things—a sample of gas *and* a collection of molecules? Again, Harré is forced to say that the coupla in such statements "expresses a relation more like that of analogy". The exact meaning of this is not clear, and this certainly does not go very happily with taking theories seriously and with such statements as "we are entitled, under some con-

[1] Place, U. T., "Is Consciousness a Brain Process?", *British Journal of Psychology*, 1956. Smart, J. J. C., "Sensations and Brain Processes", *Philosophical Review*, 1954.

ditions, to ask whether the imaginary mechanisms or model (invented to explain phenomena) exists".

The position defended by Harré is one that few philosophers support, whereas there is a great body of positivist doctrine developed with various degrees of care and sophistication. This makes it inevitable that there are many loose ends left by Harré's account. However, he has met squarely the central points of the positivist thesis and I believe that this work will significantly modify the approach of subsequent discussion on the nature of scientific theories. In particular, it is to be hoped that Harré's challenge to the complacent acceptance by scientists and philosophers alike of the Copenhagen Interpretation of fundamental physical theory, according to which it is quite legitimate to accept two incompatible theories of the same phenomena, will provoke some new thought on the question.

No. 8: THE STRUCTURE OF CHEMISTRY. By E. F. Caldin. 49 p. 3s. 6d.

The author is a practising chemist It is his belief that a study of chemistry can throw considerable light on scientific method, and he considers it strange and unfortunate that chemistry has been neglected in this regard in favour of physics.

This essay achieves its aim admirably and is very clearly written. There is an outline, with some attention to history, of the main concepts of chemistry such as "pure substance", "energy", etc. There follows an account of laws in chemistry and then a discussion of chemical theory and the light it throws on the logic of theories in general. Like Harré, the author believes that a positivist analysis of theory does not do justice to the role that theories actually play in science. The question of the existence of theoretical entities is taken up and some illuminating points are made about the way the practice of chemists modifies the hypothetico-deductive method. Caldin is not attempting to deal with the philosophical complexities that have occupied Harré; however, I find his summing up on the question "Do atoms exist?" most satisfactory. He asks "if this means 'Does anything exist corresponding exactly to the description of an atom given by modern theory?' " His answer is "Probably not. But it is important to add that something *analogous* to the modern model exists."

This book seems particularly useful for students in the philosophy of science and would serve as an excellent counter to views such as those of Duhem.

No. 6: THE IMAGE OF NEWTON AND LOCKE IN THE AGE OF REASON. By Gerd Buchdahl. 116 p. 5s.

The author deals with the influence of Newton and Locke on

the thought of the eighteenth century. He deals in turn with the somewhat rationalist impact of Newton's "Principia" with its mathematical systematising of so large a part of nature, with the rather opposite empirical tradition advocated by the two, and finally with the reaction, especially among literary figures, against these ideas in the later part of the century.

A little over half the book is occupied by illustrative texts which are an excellent complement to the first section.

SOCIAL ANTHROPOLOGY, by D. F. Pocock (No. 7; 118 p.; 5s.) is concerned particularly with the method of this science and deals historically with the development of the modern approach; while PIONEERS OF PRE-HISTORY IN ENGLAND, by L. K. Clark (No. 10; 112 p.; 5s.) points out that it was not until 1859 that scientists generally accepted that man had existed for a considerable time before the period dealt with in the Bible, and goes on to describe the discovery of the evidence and the ensuing controversies which led to this acceptance.

F. J. CLENDINNEN.

ORR. By W. H. C. Eddy. Brisbane, Jacaranda Publishers Pty. Ltd., 1961. xxxvi, 764 p.; 59/6d.

It is a very good thing for Australia that a book such as this one can be written by a responsible and reputable scholar and published by a responsible and reputable press. Mr. Eddy has made a very thorough inquiry into the mass of documentation which has accumulated round the Orr case. His inquiry has led him to a strongly held view on the truth of the matter, a view which he puts forward in this book with no mincing of words but nevertheless in a manner which does him great credit.

Whatever one's own views on the Orr case, they have to be set aside if a serious contribution to the discussion of so important a public issue is to be fairly evaluated. The putting aside of one's own views in this instance requires that it be clearly borne in mind that Mr. Eddy is throughout arguing a particular case. His thesis is that Professor Orr has been the victim not merely of a miscarriage of justice, but of a deliberate and successful conspiracy to ruin him. This does *not* mean that Mr. Eddy assumes the mantle of omniscience and purports to arrive at a final and dispassionate judgment. What it does mean is that Mr. Eddy assumes the responsibility of not knowingly manipulating the evidence so as to give an untrue impression of its effect to the reader. The author is entitled to put the most favourable construction upon the evidence, but not to falsify it. If he discharges this responsibility he cannot be fairly condemned by those who disagree with his conclusions.

In the opinion of the present reviewer Mr. Eddy's presentation of his case is competent, careful and overwhelming. A critical reading of the book reveals a number of minor weaknesses, such as a spasmodic addiction to the rhetorical question, which sometimes reduce the impact of a point well made; but there is no sign of irresponsible treatment of the evidence, either by manipulation out of context or by manifestly improper interpretation. Not everyone will agree with every inference drawn by Mr. Eddy from particular exchanges in the various proceedings, and many will think that he underplays the unwisdom of some of Professor Orr's actions and statements. But it is difficult to believe that anyone who reads this book with a genuine effort at impartiality will finish it unconvinced that, in the absence of a reasoned reply based equally carefully on the evidence, the author has proved his case.

For many people a central difficulty in condemning Professor Orr's attackers out of hand has been the failure of his action for wrongful dismissal. There is a widespread feeling that if he could not prove his case in court, then that should be the end of the matter. This point of view is likely to appeal less to lawyers, paradoxically enough, than to any other class of the community. Lawyers are perfectly familiar with the notion that a judge, or even several judges, may be wrong. There is nothing improper in advancing the view that a particular decision was wrong or in pointing out that an appeal court may have been unable for technical reasons to set the error right. In the present context there is therefore nothing improper in expressing the opinion that Orr's action should have succeeded, which is what Mr. Eddy does with a wealth of supporting argument and evidence. In this part of his argument he is powerfully supported by the findings of the Committee of Inquiry appointed by the Federal Council of University Staff Associations of Australia, which are reprinted at the back of the book. The Committee's findings also support Mr. Eddy's conclusion that quite apart from Professor Orr's guilt or innocence on the Kemp charge, he certainly did not receive a fair hearing at the hands of the University of Tasmania.

The nature of Mr. Eddy's thesis, that long before Miss Kemp appeared on the scene a conspiracy against Professor Orr was afoot, has the advantage that it compels him to put the Kemp charge in perspective. As he himself says, the Orr case is not coterminous with the Kemp case. Undoubtedly the history of Orr's relations with the University of Tasmania for years before anyone had ever heard of Miss Kemp greatly strengthen the view that he has been the victim of a monstrous injustice. This part of the book is written with the same hard-hitting but legitimate persuasiveness as the rest.

In sum, this book deserves to be read seriously by anyone who

is really interested in the future for liberal ideas in Australia, even if he has no interest at all in either Professor Orr or the University of Tasmania. For the sceptics it is worth observing that Mr. Eddy makes a large number of statements about individuals which, if untrue, are undoubtedly seriously defamatory; but no one has yet sued him for defamation.

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BOOKS RECEIVED

(Inclusion in this list neither guarantees nor precludes later review.)

BAHM, Archie J. Yoga, union with the ultimate: a new version of the ancient Yoga sutras of Patanjali. New York, Frederick Ungar Publishing Co., 1961. 162 p. $1.25 (paper covers).

BENTZ, Hans W. Karl Jaspers in Ubersetzungen; Karl Jaspers translated; Karl Jaspers traduit. (Weltliteratur in Ubersetzungen, Reihe 1, Band 1.) Frankfurt, Hans W. Bentz Verlag, 1961. 31 p. DM 9.60 (paper covers).

Bibliography of translations of Jaspers into various languages.

BRANDT, Richard B., ed. Value and obligation: systematic readings in ethics. New York, Harcourt, Brace & World, Inc., 1961. x, 715 p. $6.95.

"The readings are divided into groups corresponding to seven major problems. Each group is prefaced by an introduction, written by the editor, which explains what the major problem is and provides an initial sketch of the types of theory to come." Preface. The topics include the criteria of rightness, the justification of those criteria, relativism, and free will. The authors represented range from Plato and Aquinas to Ruth Benedict and William Graham Sumner, taking in on the way, among others, Hobbes, Mill, Ross, Ayer, Stevenson, Russell, Baier and Medlin.

CHAMBERS, Frank P. Perception, understanding and society: a philosophical essay on the arts and sciences and on the humane studies. London, Sidgwick and Jackson, 1961. 300 p. 25s. (U.K.).

Argues that there are two distinct modes of thought, "conception", whose special development is science, and "perception", which manifests itself in the arts: that "the humane studies" (history, anthropology, sociology, etc.) are arts rather than sciences; and that the recognition of "perception" as a way of knowing may solve many epistemological and metaphysical problems.

CHRISTENSEN, Niels Egmont. On the nature of meanings: a philosophical analysis. Copenhagen, Munksgaard, 1961. vi, 223 p. Danish Kr. 28. Paper covers.

"It is the thesis of this work that the meaning of an expression pertains to the capacity of that expression of being rightly produced when and where, and only when and where, something specific of a non-linguistic kind is present, be it an object, property, relation, situation, or whatever it may be." p. 14. This theory is compared and contrasted with various other theories of meaning.

FEIGL, Herbert, and MAXWELL, Grover, eds. Current issues in the philosophy of science: symposia of scientists and philosophers (Section L of the American Association for the Advancement of Science, 1959). New York, Holt, Rinehart and Winston, 1961. ix, 484 p. $6.

The broad topics covered are theory construction, empirical and conventional elements in physical theory, induction, probability and simplicity, the logic of variables and constants, quantum theory, and methodological problems of the social sciences. Contributors include Hanson, Feyerabend, Sellars, Mehlberg, Scriven, Grunbaum, Lande and others.

FREUDENTHAL, Hans, ed. The concept and the role of the model in mathematics and natural and social sciences; proceedings of the colloquium sponsored by the Division of Philosophy of Sciences of the International Union of History and Philosophy of Sciences organized at Utrecht, January, 1960. (Synthèse Library.) Dordrecht, Holland, D. Reidel, 1961. 194 p. Dutch florins 21.

Papers by Dutch, French, Belgian, German, Polish and American logicians.

GAULTIER, Jules de. From Kant to Nietzsche; translated by Gerald M. Spring. New York, Philosophical Library, 1961. xiv, 290 p. $4.75.

The first work of the exponent of *Bovarysme*, the human faculty of self-deception. "Life is illusory and fundamentally a good show." First published in 1900.

GRAHAM, A. C. The Problem of Value. London, Hutchinson University Library, 1961. 192 p. 12s. 6d. (U.K.)

Argues for objective criteria of moral and aesthetic standards, while allowing their close connection with wants and feelings. "Right and wrong change with social evolution and vary with the particular needs of societies and individuals; they are none the less objective." Well written, it ranges more widely than most contemporary books about value. The author is Lecturer in Chinese in London University.

HALL, Everett W. Our knowledge of fact and value. Chapel Hill, University of North Carolina Press, 1961. xvii, 220 b. $5.

Attempts to provide a theory of knowledge that will reconcile empiricism with a belief in the objectivity of value.

HART, H. L. A. The concept of law. (Clarendon Law Series). Oxford, Clarendon Press, 1961. x, 263 p. 39s 3d. (Australian).

Criticises the Austinian view that law consists in orders backed by threats, and goes on to discuss the relation between law and morality. Clear, readable and carefully argued, this book is a valuable contribution to moral and political philosophy as well as to jurisprudence.

HOSPERS, John. Human conduct: an introduction to the problems of ethics. New York, Harcourt, Brace & World, Inc., 1961. viii, 600 p. $6.50.

Comprehensive introductory text-book, with exercises and reading-lists. Most space is given to discussing theories about the

criteria for moral judgments (egoism, utilitarianism, deontology, etc.); there are excursions into "political ethics" (democracy, justice, equality, punishment) and free will; finally, there is a relatively brief treatment of "the problem of verification" or meta-ethics (naturalism v. non-naturalism). Readable and lively.

KLAUSNER, Neal W., and KUNTZ, Paul G. Philosophy: the study of alternative beliefs. New York, Macmillan Co., 1961. xiv, 674 p. 60s. 3d. (Australian).

An introductory text-book, with exercises and reading-lists. Begins with the distinction between belief and knowledge, and goes on to discuss logic, theories of truth, epistemology, "beliefs about reality" (materialism, idealism, etc.), "beliefs about history", "beliefs about political relations", "religious beliefs", "beliefs about aesthetic values" and ethics ("beliefs about what one should do").

MILLER, Barry. The range of intellect. London, Geoffrey Chapman, 1961. viii, 251 p. 30s. (U.K.).

Attempts to solve the puzzle about other minds by arguing that love is a means of knowing.

MUKERJEE, Radhakamal. The philosophy of social science. London, Macmillan, 1960. ix, 187 p. 37s. 6d. (Australian).

"The philosophy of social science bases its metaphysical postulates on dialectic, which is an empirical philosophy of continuous activity or becoming, an endless quest of that whole which man is and which he understands as the unity and harmony of his opposites or antinomic modes, being and becoming, reality and appearance, order and individuation, self-actualisation and self-transcendence, sensate and ideational mode." Introduction.

MYERS, Henry Alonzo. Systematic pluralism: a study in metaphysics. Ithaca, Cornell University Press, 1961. viii, 195 p. 41st. 9d. (Australian). (Our copy from Oxford U.P., Melbourne).

"The sciences and branches of knowledge are perspectives. They differ from individual and personal points of view in that they are perfectly communicable and are the result of social co-operation. One of them is not more real than another; and the truth or reality of one cannot be determined by another. They are prespectives of a Metaphysical object, but this metaphysical object is no more real than they." Introduction. Myers died in 1955. This book has apparently been put together from an unpublished doctoral dissertation together with chapters from his published works.

PAUL, Leslie. Persons and perception. London, Faber and Faber, 1961. 231 p. 25s. (U.K.).

The traditional puzzles about perception, some considerations from neurology and communications engineering, and a respect for "the most baffling and exciting of modern philosophers", Whitehead, lead the author to the conclusion that "feeling is some basic

physical rapport below consciousness or perception" and to a new (or at least unfashionable) ontology.

POLE, David. Conditions of rational inquiry. London, Athlone Press, University of London, 1961. ix, 229 p. 30s. (U.K.).

Taking moral judgments seriously as judgments, the author first considers whether they can properly be regarded either as intuitions or alternatively as sentiments, attitudes, or decisions. He then proceeds to consider the relations between judgment and evaluation, and the place of evaluation in inquiry.

RANKIN, K. W. Choice and chance; a libertarian analysis. Oxford, Blackwell, 1961. 182 p. 18s. (U.K.).

Accepting free will, the author explores its implications for such concepts as causation and responsibility. Closely argued but rather tortuous.

SCHELER, Max. Ressentiment; edited, with an introduction, by Lewis A. Coser; translated by William W. Holdheim. New York, Free Press of Glencoe, 1961. 201 p. $4.

First published in German in 1912; revised and enlarged, 1915. "*Ressentiment* denotes an attitude which arises from a cumulative repression of feelings of hatred, revenge, envy and the like [and] leads to a tendency to degrade, to 'reduce' genuine values as well as their bearers. As distinct from rebellion, *ressentiment* does not lead to an affirmation of counter-values since *ressentiment*-imbued persons secretly crave the values they publicly denounce." Nietzsche, who coined the term, uses it to attack "the slave morality" of Christianity; Scheler argues that *ressentiment* is rooted in "not Christian values but the bourgeous morality which slowly began to erode Christian morality since as early as the thirteenth century".

SCHMIDT, Paul F. Religious knowledge. Glencoe, Ill., Free Press, 1961. ix, 147 p. $4.

Examines the claims to knowledge made by the more important religions in the light of the accepted criteria for knowing. Concludes that religious statements are essentially the expression of attitudes.

SHACKLE, G. L. S. Decision, order and time in human affairs. Cambridge, University Press, 1961. xiv, 302 p. 35s. (U.K.)

"Decision is more than mere response to circumstances and contains an element which we may call inspiration . . . Secondly, in analysing decision the use of a distributional uncertainty variable, that is, probability, becomes in principle inappropriate and must give way to a non-distributional variable such as possibility, understood as discriminable in some manner into degrees . .. " Preface.

SHEPARD, Paul. Psycholex. New York, Megwa, 1960. x, 171 p. $6.

An attempt to apply psychiatry not only to criminal behaviour, but also (on the assumption that a state is a genuine person) to international affairs. For example: Transvestism: "Case 27: State X,

known as a docile, passive type of personality ('female') appears out of its character in military dress ('masculine') and refuses innocent passage to a vessel of State Y on an international river within the territory of State X." Paedophilia: "Case 28: State X has a record of recognizing 'infant' States, though later loses interest as these States mature.' Homosexuality: "Case 29. State X, a military type of personality, seeks intercourse only with similar types of States."

SINGER, Marcus George. Generalization in ethics: an essay in the logic of ethics, with the rudiments of a system of moral philosophy. New York, Alfred A. Knopf, 1961. xii, 351, x p. Price not given.

Maintains that the argument "If everyone did that, the consequences would be terrible; therefore, no one ought to do that" is "perfectly valid when properly used and qualified, is central and decisive in moral reasoning, and if fundamental in any adequate moral philosophy". Hence it may be made the basis of a rational normative system of ethics.

SMART, J. J. C. An outline of a system of utilitarian ethics. Melbourne, Melbourne University Press on behalf of the University of Adelaide, 1961. viii, 51 p. 5s. 6d. (Australian). Paper covers.

While assuming "a non-cognitivist meta-ethics", the author defends utilitarianism as a "conceptually clear and emotionally attractive system of normative ethics". Reviewed in this issue.

WILSON, John. Reason and morals. Cambridge, University Press, 1961. vii, 187 p. 15s. (U.K.)

Argues that the analysis of moral language has value and importance for the layman as well as the philosopher, and attempts to provide such an analysis for the general reader.

NOTES AND NEWS

AUSTRALASIAN ASSOCIATION OF PHILOSOPHY

ANNUAL CONGRESS, 1961

The Congress for 1961 was held in Canberra between August 21st and 25th. The programme was as follows:

Monday, August 21st:

8 p.m. Presidential Address: Professor K. E. M. Baier: "Pains".

Tuesday, August 22nd:

10 a.m. Professor D. S. Shwayder: "Men and Machines".

2 p.m. Mr. P. E. Hutchings: "'Is' and 'Ought' Again".

4.30 p.m. Council Meeting.

7.15 for 7.45 p.m. Annual Dinner.

Wednesday, August 23rd:

10 a.m. Mr. P. C. Gibbons: "Individuals".

5 p.m. Annual General Meeting.

8 p.m. Mr. F. J. Clendinnen: "Explanation in Terms of Wholes and Parts".

Thursday, August 24th:

10 a.m. Mr. J. Kovesi: "Situations and Principles".

8 p.m. Professor J. J. C. Smart: "Philosophy in the Light of Modern Science".

Friday, August 25th:

10 a.m. Mr. W. J. Ginnane: "The Possession of Concepts".

ERRATUM

On p. 149 of D. R. Duff-Forbes's article "Theology and Falsification Again" (Vol. 39, No. 2, Aug. 1961), 4 lines from the end of the second last paragraph,

for 'certainly was not an admission that the . . . .'
read 'amounted to no more than this: "the . . . .'